Essays in State Policy

By

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Essays in State Policy

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Abstract

This dissertation consists of three chapters each examining different areas of state policy. Each chapter revolves around an examination of different approaches taken by states in a particular area of policy. The questions answered arise in the contexts of income taxation, environmental regulation, and social insurance. Chapter 1 looks at impacts of a particular policy in one state by comparing that state to select other states. Chapter 2 looks at factors influencing the adoption of a policy by a group of states, and aspects of implementation. Chapter 3 looks at the relationship between variations in a policy applied in all states and differences in related outcomes. Each chapter makes distinct and novel contributions in its respective context.

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Chapter 1

The Kansas Tax Experiment: Impact of 2012 Kansas Tax Reform on Output, Employment & Establishments

1.1 Introduction

In 2012, Kansas enacted major tax reform, with primary goals of promoting economic growth and job creation. The Governor described the reform as a "real live experiment" and predicted it would be "like a shot of adrenaline into the heart of the Kansas economy" (MSNBC 2012). Job creation was a major theme in promoting the new tax package, with emphasis placed on small and new businesses. At the signing ceremony, the Governor announced: "Today's legislation will create tens of thousands of new jobs and help make Kansas the best place in America to start and grow a small business." A state representative proclaimed: "Kansas is embarking on and setting the threshold for the nation with a pro-growth, pro-jobs tax reform policy. Lowering taxes on individuals and small businesses will jump start the private sector growth in Kansas, allowing Kansans to grow Kansas." A media release provided that: "Dynamic projections show the new law will result in 22,900 new jobs, give $2 billion more in disposable income to Kansans and increase population by 35,740, all in addition to the normal growth of the state" (Kansas Office of the Governor 2012).[1]

Major facets of the reform were decreasing the individual income tax rates (from 3.5 to 3 percent, and from 6.25 and 6.45 to 4.9 percent), and a 'business income exclusion,' which

[1] The time frame for those projections was not provided in the release, but appears to have been by 2020.

essentially excluded self-employment, pass-through business, rental real estate, royalty, and farming incomes from the state income tax.[2] These changes took effect at the start of 2013. Corporate income taxes did not change, although corporate rates were cut in prior years.

The legislation included limited revenue increasing measures. Additional base-broadening measures originally proposed were cut from the legislation prior to enactment. Surplus funds were initially available and new revenue was expected from casinos. But there was no clear plan to offset the decline in revenue expected to accompany the tax cuts. This has been a major criticism of the policy. Because Kansas has a constitutional mandate requiring a balanced budget, it also ensured that future changes (on the spending side, the revenue side, or on both sides) would be necessary.[3]

Initial estimates from the Kansas Legislative Research Department of the reforms expected impact on State General Fund receipts were that it would result in a net lost tax revenue of $231.2 million for fiscal year 2013 (only partially overlapping policy effective dates), $802.8 million for fiscal year 2014, and greater in each of the next four fiscal years. The six-year total estimated net lost revenue was $4,539.1 million (Kansas Legislative Research Department 2012). Figure 1.1 plots annual state-level individual income tax collections in Kansas from 1994 to 2015. For comparison, averages from two groups of regional states, and from all U.S. states are also plotted.[4] Following the reform, individual income tax collections in Kansas sharply decline, while comparison group means continue to rise. Total tax revenue

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[2] More precisely, it fully excluded from the state income tax all income reported on lines 12, 17, and 18 of a taxpayers' federal return form (1040).

[3] Theoretically, a tax cut could "pay for itself" if it results in increased economic activity and taxes imposed on that increased activity exceed declines from the cut. However, based on the legislative record and official statements relating to the policy changes, it does not appear that this type of effect was envisioned. The initial inclusion of base broadening provisions further signals an understanding that the enacted provisions were not going to finance themselves. And the projected economic benefits (in terms of job and population growth), even under favorable estimates, would not bring revenue gains in excess of the losses.

[4] The regional groups are: (1) the four states that border Kansas: Colorado, Missouri, Nebraska, and Oklahoma, and (2) a slightly modified version of those states, replacing Colorado with Iowa, which is believed to be more similar to Kansas.

also initially declines, although not as sharply. From 2012 to 2013, individual income tax revenue declined 19.4 percent while total tax revenue declined 4.5 percent.

Previous work examining impacts of the same tax legislation focuses on the the business income exclusion (DeBacker et al. 2016; Turner and Blagg 2017). This paper adds to their findings by examining additional outcomes and by considering the effects of the entire policy, which, in addition to the business income exclusion, included a substantial drop in the top marginal individual income rate. It uses the synthetic control method (SCM) to analyze the impact on real gross state product (RGSP) per capita, employment, and the number of business establishments. Results suggest that the tax cuts did not have the desired impact on most of the outcomes examined.

Section 2 of this paper briefly discusses select related literature. Section 3 provides additional background surrounding the policy changes of interest. Section 4 describes the empirical framework used in evaluating the policy changes. Section 5 describes the data and samples. The remaining sections present empirical results and conclusions.

1.2 Literature Review

As mentioned, the two major components of the 2012 Kansas tax reform were: (1) the decrease in individual income rates, and (2) the business income exclusion. Both are components of the individual income tax system, but theoretically apply to different types of activity. This section discusses select literature on impacts of individual and business income taxes on economic outcomes, focusing primarily on state-level taxes. It then discusses two other papers that look at impacts of the 2012 Kansas tax reform.

Empirical evidence on the efficacy of tax cuts as a policy tool for job creation is mixed. Theoretically, the impact is ambiguous. In the context of state corporate income taxes, Ljungvist and Smolyansky (2014) find evidence of asymmetric results. In particular, they find that a one percentage point increase in the top marginal state corporate income tax rate reduced employment by 0.3 to 0.5 percent (and income by between 0.3 and 0.6 percent), measured relative to neighboring counties on the other side of the state border. Rate decreases, on the other hand, only significantly impacted employment and income during recessions.

Shuai and Chmura (2013) find evidence that state corporate income tax rate changes produce short run, transitory impacts. They find significant impacts on state employment growth, observed primarily in the first year. Results indicate that the act of cutting alone (measured by a binary indicator) has a significant positive impact in the year of the cut, an insignificant positive effect the year after, and basically no impact in subsequent years.

At the federal level, Mertens and Ravn (2013) find evidence that corporate income tax rates impact GDP and investment but not employment or consumption. Specifically, they find that a one percentage point cut in average federal corporate income tax rates (measured as the ratio of aggregate federal corporate profit tax receipts to aggregate corporate profits) increased GDP by 0.4 to 0.6 percent in the short-run, but had no immediate impact on employment or hours worked. Cuts increased private sector investment but had no impact consumption.

The two other studies analyzing the Kansas reform have focused on the business exclusion. DeBacker et al. (2016, 2017) analyze amounts reported in different categories on individual federal income tax returns. They are able to identify and find evidence of income shifting separate from real impacts. Turner and Blagg (2017) measure impacts on aggregate measures of employment and proprietors, but focus more narrowly on the base change impacts; namely on the business income exclusion.[5] They consider two outcomes (employment and proprietors) each measured three different ways (log, per capita, and growth rate), using two samples (all counties in the four border states and border county pairs along the Kansas border) and two pre-intervention periods (one beginning in 2004, the other in 2010). For

[5] They focus on base changes by controlling for the rate changes.

all counties, starting in 2004, they find statistically significant negative impacts on log and per capita employment. Starting in 2010, estimated impacts for both remain negative but are not statistically significant. Estimated employment growth impacts are positive but not statistically significant. Their estimated level impacts for proprietors are all negative and not statistically significant.

1.3 Policy Change Details and Background

The 2012 tax legislation (HB 2117) was enacted in May of 2012, and became effective July 2012. Most (if not all) of the tax provisions were written to apply beginning in the 2013 tax year. Tax reform was identified as part of the political agenda at least as early as January 2011.[6] The 2012 legislation was followed with additional tax legislation in 2013, 2014, 2015, and 2017.

The reform decreased individual income tax rates for all taxpayers, collapsed the number of brackets from three to two, and increased the standard deduction for joint and head of household filers. For the top bracket, the rate dropped from 6.45 and 6.25 percent to 4.9 percent. For the lower bracket, the rate dropped from 3.5 to 3 percent. The new business income exclusion subtracted amounts reported on federal 1040 lines 12, 17, and 18 from income for the purposes of the state income tax. Those lines correspond to business income, rental real estate, royalties, partnerships, S corporations, trusts, and farm income. Revenue increasing measures reduced and eliminated a handful of credits and refunds, and provided for a gradual reduction (partial phase-out) in itemized deductions for individual taxpayers. The changes also eliminated a severance tax exemption.

[6] For example, the Governors State of the State Address from January 2011 noted a tax policy agenda. ("And for all of this to work, we need a tax code that encourages investment, income growth, and job creation. I pledge to work with the Legislature on resetting our tax code, particularly with an eye toward lowering income tax rates. In general, my Administration's first priority will be creating jobs that provide more income and opportunity for Kansas families. ... The days of ever expanding government are over and under my administration, they will not return.")

As mentioned in the introduction, the 2012 reform did not substantively modify the corporate income tax. As a result, direct labor demand effects from the tax policy changes should be limited to only noncorporate entities (more precisely, to entities not taxed as corporate entities, including S-corporations). Noncorporate employment was approximately 38 percent of total employment on average from 2010 to 2012 in Kansas (County Business Patterns, annual state-wide numbers).

A series of top corporate income rate cuts were phased in from 2008 to 2011. The top corporate income rate dropped from 7.35 to 7.1 percent for 2008, then to 7.05 percent for 2009 and 2010, and finally to 7 percent for 2011 and beyond. Corporate franchise tax rate reductions were phased in over the same period, and the applicability threshold was increased. If those corporate tax changes, taking effect from 2008 to 2011, affected economic activity (relative to control groups), they will confound difference-in-differences estimates of the 2012 reform's impact on that activity.

1.4 Empirical Approach

I estimate impacts of the Kansas tax reform on output, employment, and establishments using the synthetic control method (SCM). The SCM is introduced in Abadie and Gardeazabal (2003), and expanded on by Abadie, Diamond and Hainmueller (2010, 2015). In the tax policy context, it has been used to evaluate impacts of flat tax reforms (Adhikari and Alm 2016). The SCM is particularly well suited for examining impacts of a single policy intervention on aggregate outcome variables. It also removes some of the arbitrariness involved in selection control units. This section introduces the empirical framework and synthetic control estimation.

A synthetic control is a weighted average of outcome values from a set of potential control units. The set of potential control units is referred to in the literature as the "donor pool." Using a set of predictor variables, weights are assigned to each state in the donor pool so that the resulting synthetic control matches the treated state as closely as possible during a pre-intervention period. The following framework described is based on Abadie, Diamond and Hainmueller (2010).

States $s = 1, \dots, S+1$, are observed for time periods $t = 1, \dots, T$. The first state ($s = 1$) is Kansas (or more generally the treated state). The S remaining states form the donor pool. The policy change of interest occurs at time $T_0 + 1$, so that $t = 1, \dots, T_0$ indexes the pre-intervention period, and $t = T_0 + 1, \dots, T$ indexes the post-intervention period. Y_{st} denotes the outcome of interest for state s at time t. The effect of the policy intervention for unit s at time t is specified in a potential outcomes framework as $\alpha_{st} = Y_{st}^T - Y_{st}^0$, where Y_{st}^0 denotes the outcome that would be observed without the intervention.

$$Y_{st} = Y_{st}^0 + \alpha_{st} D_{st},$$

where

$$D_{st} = \begin{cases} 1 & \text{if } s = 1 \text{ and } t > T_0 \\ 0 & \text{otherwise.} \end{cases}$$

Each state in the donor pool is assigned a non-negative weight w_s, such that the combined weights for all states in the donor pool sum to one. The weights collectively form a $(S \times 1)$ vector W. The weights defining a given synthetic control W^* are chosen to minimize:

$$||X_1 - X_0 W||_V = \sqrt{(X_1 - X_0 W)' V (X_1 - X_0 W)},$$

where X_1 is a vector of predictor variables for the treated unit, X_0 is a matrix containing the same predictor variables for each of the donor pool units, and V is a symmetric, positive semidefinite matrix of weights assigned to the predictor variables. The predictor variable weights are assigned to reflect the relative importance of each predictor variable in predicting the outcome of interest. This can be done in different ways.[7] I solve for both the donor pool and predictor variable weights using the Synth package in R.

Given W^* and a matrix Y_0 containing the outcome variable values for each donor pool unit in each time period, the counterfactual outcome path is $Y_1^* = Y_0 W^*$. The estimated policy impact is given by the difference between that counterfactual outcome path and the observed values for the treated unit following the policy intervention. Dynamic treatment effects for year $t \in \{T_0 + 1, \cdots, T\}$ are given by:

$$\hat{\alpha}_{1t} \; = \; Y_{1t} \; - \; \sum_{s=2}^{S+1} w_s^* Y_{st},$$

where, as indicated above, $s = 1$ is Kansas, $s \in \{2, \cdots, S+1\}$ are the donor pool states, and T_0 is the number of pre-intervention years. The average treatment effect (ATE) is given by:

$$ATE \; = \; \frac{1}{T \; - \; T_0} \sum_{t=T_0+1}^{T} \hat{\alpha}_{1t}.$$

Outcomes in both the treated state and donor pool states are assumed to follow a linear factor model:

$$Y_{st}^0 \; = \; \delta_t + \theta_t Z_s + \lambda_t \mu_s + \varepsilon_{st},$$

where Y_{st}^0 is the outcome absent the intervention, δ_t are unknown common factors with constant loadings across states (common time effects), θ_t is a vector of unknown loadings (parameter), Z_s are observed covariates not affected by the intervention, λ_t are unobserved common factors, μ_s are unknown factor loadings, and ε_{st} are unobserved, mean zero, state

[7] Abadie and Gardeazabal (2003) select predictor variable weights such that the outcome variable path for the treated unit during the pre-intervention period is best reproduced by the resulting synthetic control. Abadie, Diamond and Hainmueller (2015) apply a cross-validation method to choose the predictor variable weights.

level transitory shocks. When $\lambda_t = 1$ and $\mu_s = \delta_s$, the model simplifies to a two-way fixed effects model.

Unlike difference-in-difference estimates, synthetic control estimates allow for time varying heterogeneity in unobserved variables (i.e., do not require the parallel trends assumption). Abadie, Diamond and Hainmueller (2010) show this in the context of the above specified model. When the number of pre-intervention periods is large relative to the size of the error, the bias from time varying heterogeneity approaches zero. Additional identification assumptions include that untreated donor pool states are not affected by the same intervention (no spillover effects) or by similar interventions, and that the intervention has no effect on the outcome before being implemented (no anticipation effects).

To minimize the potential for biased estimates, I use a ten year pre-intervention period. To eliminate anticipation effects, I do not include 2012 in the pre-intervention period when finding synthetic control weights.[8] Because the policy had not yet taken effect, I also do not include 2012 in calculating treatment effects. At the state-level, spillover effects are not expected to be substantial. To eliminate states affected by similar interventions from the donor pool, as described in the next section, I impose a restriction based on top marginal income tax rate changes.

Not all states will be well matched by synthetic controls. For example, if Kansas had observed outcome values strictly greater than all donor pool state values, the Kansas values would not be reproducible as a convex combination of the donor pool values. There is also a risk of interpolation bias if states in the donor pool are not similar enough to the treated state (Abadie, Diamond and Hainmueller 2010). Graphical results are presented to facilitate evaluation of the match. Additionally, following Abadie, Diamond and Hainmueller (2015),

[8] The legislation was enacted by mid 2012. At that point the changes were certain. While it did not become effective until 2013, people may have started changing their status or behavior in anticipation of the policy during 2012. For example, employees wanting to change status to an independent contractor could have done so during the end of 2012, so as to benefit immediately once 2013 began.

I compute the root mean square prediction error (RMSPE) over the pre-intervention period as a measure of goodness-of-fit. The formula is:

$$RMSPE = \sqrt{\frac{1}{T_0}\sum_{t=1}^{T_0}\left(Y_{1t} - \sum_{s=2}^{S+1} w_s^* Y_{st}\right)^2}.$$

A disadvantage of the synthetic control method is the lack of formalized inference. Placebo methods are used instead. Placebo tests are run for every state in the donor pool. A synthetic control is constructed for each state yielding a distribution of placebo effects. Donor pool states, having not been subject to the intervention, should not have large estimated treatment effects. The distribution of placebo effects is used to calculate empirical p-values for average and dynamic treatment effects. The formula used is:

$$p_1 = \frac{\sum_{s=2}^{S+1}\{\hat{\alpha}_s \geq \hat{\alpha}_1\}}{S}.$$

These values indicate the chance of estimating an effect as large as that actually estimated. The procedure used largely follows Abadie, Diamond and Hainmueller (2010), with the exception that I do not include Kansas (the treated state) in the donor pool for the placebo synthetic controls.

Placebo estimates that do not fit well in the pre-intervention period are not expected to fit well in the post-intervention period. Further they are not expected to be informative of the chance of estimating an effect as large as that estimated for a state with a closer pre-intervention fit. To adjust for this, thresholds based on pre-intervention can be used as restrictions. Abadie, Diamond and Hainmueller (2010) produce multiple sets of placebo tests using three different cutoffs, excluding states with a pre-intervention MSPE of: (i) more than 20 times the treated state MSPE, (ii) more than 5 times the treated state MSPE, and (iii) more than twice the treated state MSPE. An alternative, that avoids choosing a cutoff, is to look at the ratio of the post to pre intervention MSPE, as is done in Abadie, Diamond and Hainmueller (2015). This is the approach I follow. For each state in the donor pool, I compute a placebo RMSPE ratio:

$$Ratio_{si} \;=\; \frac{RMSPE_{Post,si}}{RMSPE_{Pre,si}} \;=\; \frac{\sqrt{\frac{1}{T-(T_0+1)}\sum_{t=T_0+1}^{T}\left(Y_{si,t} \;-\; \sum_{s\neq si}^{S+1} w_s^* Y_{st}\right)^2}}{\sqrt{\frac{1}{T_0}\sum_{t=1}^{T_0}\left(Y_{si,t} \;-\; \sum_{s\neq si}^{S+1} w_s^* Y_{st}\right)^2}}.$$

Ratio values for each state are used to rank the treated state and calculate an empirical "p-value."

Placebo tests cannot rule out the possibility that the estimated impact is driven by another cause. Specifically, they cannot rule out the presence of idiosyncratic shocks or other policy changes. The placebo method does not reflect this source of uncertainty. While the SCM is said to be a more "data driven" approach, and in some senses is, it still requires selecting donor pool members, predictor variables, and the pre-intervention period to use in optimizing weights. Each of these selections presents an opportunity for difficult and possibly arbitrary decisions that could end up driving analysis results.

Difference-in-differences estimates are based on the following standard model,

$$Y_{st} = \alpha\ (KS_s \times Post_t)\ +\ u_s\ +\ v_t\ +\ \epsilon_{st},$$

where α is the policy treatment effect, Y_{st} is the outcome variable, u_s are state fixed effects (which absorb state level differences that remain constant over the period examined), and v_t are time fixed effects (which absorb differences over time that effect the states in the same way). KS_s is an indicator equal to one for Kansas. $Post_t$ is an indicator equal to one for observations in 2013 or later. Identifying assumptions include that the treated and control states follow parallel trends in the outcome variable, no spillover effects, and no anticipation effects.

The difference-in-differences analysis uses a control group primarily based on geographic proximity. Four states border Kansas: Colorado, Missouri, Nebraska, and Oklahoma. I use Iowa, Missouri, Nebraska, and Oklahoma, in essence replacing Colorado with Iowa.[9] Anecdotally, Kansas and Colorado are expected to differ in a number of important respects. Colorado is a popular tourist destination with winter and summer attractions. Kansas is not. Additionally, legalization of marijuana for recreational use, a potentially important positive economic shock, became effective in Colorado at the end of 2013.

1.5 Data

This section introduces the data and samples used in the empirical analysis. Analysis is done at the state-year level, from 2001 to 2015.

1.5.1 Outcome Measures

Real gross state product (RGSP) is from BEA's Annual Gross Domestic Product by State data. Positive impacts from tax cuts could be directly offset in RGSP by accompanying decreases in government spending. To evaluate this, I adjust RGSP by subtracting off the public sector component. In Kansas, the pubic sector accounted for approximately 15.1 percent of annual RGSP on average from 2000 to 2011. Results are reported for both the original and adjusted measures.

Employment is from the BEA State Personal Income accounts. It includes wage and salary employment, as well as proprietor employment. It also includes farm employment, an

[9] Admittedly, this makes the control group selection more ad-hoc than it would be relying solely on geographic proximity and selecting the bordering states. However, comparing means of each outcome variable for Kansas, the border state group, and the adjusted version demonstrates that the adjusted version is more similar to Kansas than the border version.

important sector in Kansas that is not included in all measures of employment.

Establishment data is from the US Census County Business Patterns (state-level files). A separate measure of establishments, establishments with no employees is also considered. Nonemployer establishments data is from the US Census Nonemployer Statistics (NES). Nonemployers are not counted in either establishments or employment.

1.5.2 Predictor Variables

Predictors variables used in constructing synthetic controls vary some by outcome but mostly overlap. The specific predictors for each outcome and resulting weights are reported with results. They include sector shares, demographic, labor market, and human capital measures, which are related to economic growth and similar to predictor variables included in other studies (Abadie and Gardeazabal 2003; Abadie, Diamond, and Hainmueller 2015). Sector shares are calculated based on RGSP. Population, gender, age, and population density data are from the Census. Unemployment rate, labor force participation rate, education, and workforce skill level data are from individual-level Current Population Survey microdata, aggregated to the state-year level. Several, but not all, lags of the outcome variables are included. Alternative approaches are to include the average, or the last observed value.

1.5.3 Donor Pool

The donor pool is selected from the 50 US states. States without individual or corporate income taxes are excluded, as are states that had, in a single year, corporate or individual income rate changes at or above a one percentage point threshold.[10] Policy thresholds have

[10] State data on the top corporate income tax rate, the top individual income tax rate, and the sales tax rate for 2000 to 2015 are from the Tax Foundation. The rate data is not perfect. For example, in the case of Kansas it leaves out the decrease in the top corporate income tax rate from 7.35 percent to 7.1 percent taking effect in 2008.

been used in other contexts to decide which groups to include in a donor pool.[11] Louisiana is also excluded. The top rate in Kansas dropped by 1.55 beginning in 2013, well above the threshold. By contrast, none in the series of small corporate income rate cuts between 2007 and 2011 exceed this threshold. Nor do any of the small individual rate cuts that took effect in Kansas after 2013.

From 2001 to 2015, 105 changes in top individual income rates (77 decreases and 28 increases), and 80 changes in top corporate income rates are observed. Decreases were more frequently observed, but increases were more likely to exceed one percentage point. 15.58 percent of the individual and 25 percent of the corporate rate decreases were one percentage point or higher.[12] 39.29 percent of the individual and 60 percent of the corporate rate increases were one percentage point or higher.[13] These exclusions leave a baseline donor pool of Arkansas, Colorado, Georgia, Idaho, Iowa, Massachusetts, Maine, Mississippi, Missouri, Nebraska, Oklahoma, Pennsylvania, South Carolina, and West Virgina. Figure 1.2 shows Kansas in gray and each of the donor states in blue.

1.5.4 Time Frame

Analysis is done at the state-year level from 2001 to 2015. Following Abadie, Diamond and Hainmueller (2010) and others, I use ten years of pre-intervention data: from 2001 to 2011. Longer time periods should reduce the potential bias from time-varying unobservable effects (as described above, this source of bias approaches zero as the pre-intervention period increases). As explained above, 2012 is not included in the pre-intervention period due to concern about anticipation effects.[14]

[11] For example, in considering the impact of a large scale Tobacco control program implemented in California, ADH 2010 used having had a state per pack cigarette tax increase of 50 cents or more as a threshold for excluding states.

[12] Respectively, 12 out of 77 and 15 out of 60.

[13] Respectively, 11 out of 28, and 12 out of 20.

[14] The reform was fully enacted by the middle of 2012, leaving time for taxpayers to plan for and potentially change activity in anticipation of the changes taking effect.

1.6 Empirical Results

This section describes the empirical results. The results for the first outcome, RGSP per capita, are explained in greater detail than the others. Similar explanations hold for other outcomes. Tables 1.1 and 1.2 report donor pool and predictor weights for the three baseline outcomes (real GSP per capita, employment, and establishments). Table 1.3 reports pre-intervention averages in KS, the donor pool states, and for the weighted synthetic control for each outcome.

1.6.1 Real Gross State Product Per Capita

Figure 1.3 presents results for RGSP per capita in four graphs. Figure 1.3(a), shows actual RGSP per capita in Kansas (in blue) and for the estimated synthetic control for Kansas (in gray). The dashed vertical line (in red) marks the beginning of 2013. The difference between the synthetic control and Kansas before 2013 indicates how well the synthetic control fits, with a closer match indicating a better fit. The gap between the synthetic control and Kansas after 2013 is the estimated policy impact. Figure 1.3(b) plots the gap between Kansas and the synthetic control. The gap indicates that RGSP per capita decreased following the tax cuts relative to the synthetic control. However, the timing of the divide between Kansas and its synthetic raises a question about whether the gap is driven by something other than the policy intervention. Furthermore, large gaps between Kansas and the synthetic control prior to the policy changes raise a question about how well the synthetic control fits this Kansas data. Figure 1.3(c) shows the placebo analysis results. For each state in the donor pool, the gap between the observed state data its synthetic control is shown in gray. The gap for Kansas is shown in blue. After the policy change, Kansas lies below all but one other state. Figure 1.3(d) plots the post to pre RMSPE ratio for Kansas and all other states in the donor pool. Two states have values greater than Kansas. Table 1.4 reports the estimated treatment effects, pre and post RMSPE, ratio, and p-value. The average treatment effect is approximately negative 2,999 per capita. This is economically substantial - almost $3,000 per person, per year. However, the number is not statistically significant applying the ratio test of Abadie, Diamond, and Hainmueller (2015).

Excluding the public sector component of RGSP makes little difference. Figure 1.4 compares the total RGSP (solid lines) and private sector (dashed lines) results. The private sector results closely mirror overall results. Table 1.4 includes estimates for the private sector version.

1.6.2 Employment

Graphical results for total employment are presented in Figures 1.6(a) through 1.6(d). Figure 1.6(a) suggests total employment in Kansas declined following the tax cuts relative to the synthetic control. The ratio distribution indicates that this result is significant at a ten percent level. However, again the gap appears to begin before 2013. Figures 1.7(a) and 1.7(b) show results for the proprietor and the wage and salary components of total employment. Figure 1.7(b) shows a negative and significant impact on wage and salary employment similar to that observed for total employment. Figure 1.7(a), however, shows a positive impact on proprietor employment. The results are summarized in Table 1.5. As a robustness check, results for a second measure of employment is also included in Table 1.5. The second measure is from Census County Business Patterns data. It does not include agriculture, government, or self-employment. Results are similar to those from the primary measure, although are not significant. The dynamic treatment effects move in different directions as compared to the primary employment measure. This could reflect heterogeneous impacts in industries not included in the later measure. Graphical results for the later measure are presented in Figures 1.8(a) through 1.8(d).

1.6.3 Establishments

Figures 1.9(a) through 1.9(d) show the synthetic control results for establishments. The results indicate that the number of establishments declines relative to the synthetic control, although the gap appears to begin prior to the tax cuts. Based on the ratio distribution, the result is not significant. Figures 1.10(a) through 1.10(d) show synthetic control results for nonemployer establishments, which are not included in the establishments measure. Nonemployer establishments are businesses with no payroll and at least $1,000 in annual revenue. The results indicate a positive but not significant impact on nonemployer establishments similar to that observed for proprietor employment. Table 1.6 reports estimated treatment effects, RMSPEs, ratio, and p-values for both establishments and nonemployer establishments.

1.6.4 Difference-in-Difference Estimates

Table 1.7 presents difference-in-difference estimates for real GSP per capita, employment, and establishments. Results are mostly not significant. Note that while coefficient magnitudes are large, standard errors are comparably large.

1.7 Conclusion

Results indicate that the Kansas tax experiment did not have the impact politicians had hoped for. Overall the results suggest that the reform did not have a positive impact on state-level economic outcomes. They indicate potentially negative impacts on the three main outcomes considered. The findings are largely consistent with those in other papers looking at the same Kansas tax reform (DeBacker et al. 2017; Turner and Blagg 2017). The results are also consistent with empirical findings that tax cuts are less effective during expansionary phases of the business cycle (Ljungvist and Smolyansky 2014). They are less consistent with expectations based on economic theory, which suggests some channels through which a tax cut could have negative impacts on economic activity, but more generally associates tax cuts with positive impacts.

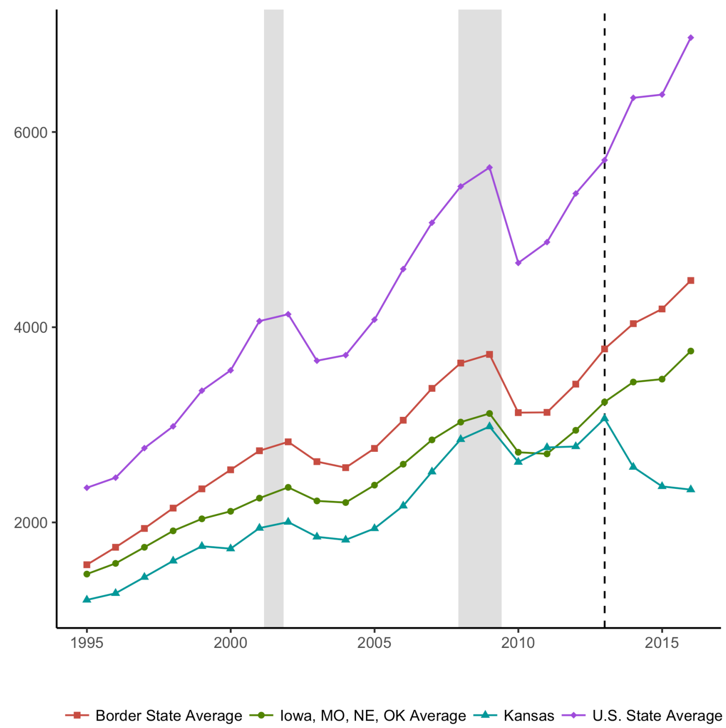
I find evidence of positive impacts on proprietor employment and on nonemployer establishments. This is consistent with individuals and firms restructuring economic activity, as suggested in DeBacker et al. (2017). It is also consistent with expansionary activity, in the form of individuals starting new businesses, and with the policy having had a positive impact on small businesses.

A limitation of the method used is that it cannot rule out the possibility that other changes at or around the same time caused the observed differences in outcomes relative to synthetic controls. Some of the synthetic control graphs suggest that there may have been an impact from prior to the tax reform taking effect, particularly in 2010 or 2011. Factors exogenous to tax policy, occurring around the time of or after the reform, could have influenced the observed results. Around 2013, a large manufacturer in the aviation industry relocated to another state. The firm had more than 2,000 employees in Kansas when it announced its relocation (The Wichita Eagle 2014). It is unlikely that the relocation was related to the tax reform, though it would have directly and indirectly impacted economic activity in the state around the same time. Exogenous volatility in energy markets and changes in the agricultural sector could also have impacted results.

Analysis of private sector measures of RGSP and employment suggest that the results are not directly explained by a corresponding decline in government spending. However, indirect effects of decreasing government spending could have influenced results. Subsequent tax changes, such as the state sales tax increase, could also have influenced results.

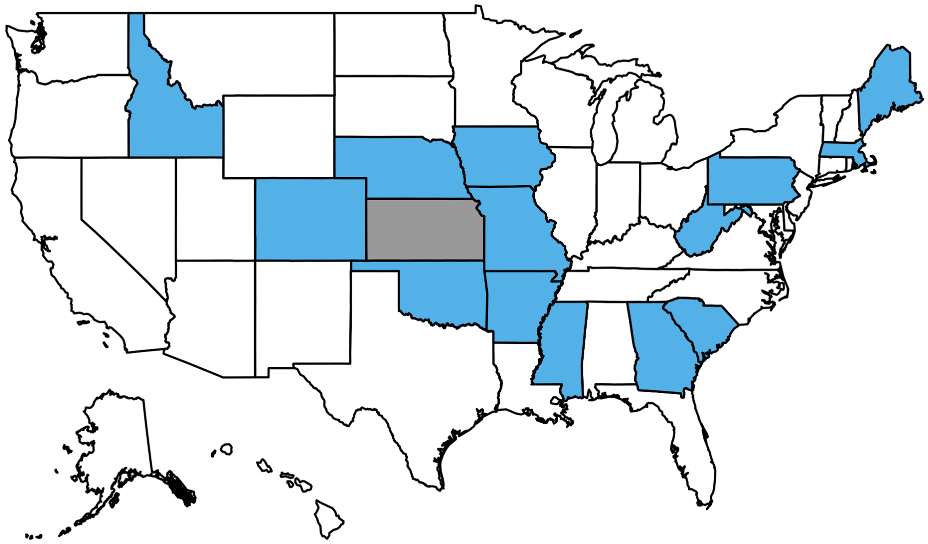
1.8 Tables and Figures

Figure 1.1: Individual Income Tax Revenue, Annual State Average (millions/year)



Dashed vertical line marks the beginning of 2013 (policy effective date). Source: U.S. Census Bureau and author calculation. Shaded areas reflect NBER recession dates.

Figure 1.2: Map of Donor Pool States



Gray = Kansas. Blue = donor pool states.

Table 1.1: Synthetic Control Weights

State	RGSP	Emp	Estab
Arkansas	0	0	.001
Colorado	0	0	0
Georgia	0	0	0
Idaho	0	.001	.062
Iowa	0	.005	.329
Massachusetts	0	.214	.252
Maine	.197	.190	.142
Mississippi	0	.001	.001
Missouri	0	0	.001
Nebraska	.103	.457	.138
Oklahoma	.700	.132	.003
Pennsylvania	0	0	.001
South Carolina	0	0	.001
West Virgina	0	0	.069

See text for additional details.

Table 1.2: Synthetic Control Predictor Weights

(a) Real GSP Per Capita		(c) Establishments	
Sector share 6	.209	Sector share 7	.245
Sector share 2	.159	Establishments, 2001	.207
Prime age male	.130	Middle skill workforce	.166
Bachelors or higher	.127	Prime age male	.136
RGSP per capita, 2003	.112	Sector share 1	.064
Population growth	.111	Employee compensation per capita	.064
RGSP per capita, 2008	.084	Establishments, 2009	.053
RGSP per capita, 2007	.023	Establishments, 2005	.030
Labor force participation rate	.013	Population density	.022
RGSP per capita, 2001	.013	Establishments, 2008	.013
Sector share 7	.007	High school or lower education	0
High school or lower education	.004	Bachelors or higher	0
Sales tax rate	.002	Labor force participation rate	0
Middle skill workforce	.002	Population growth	0
RGSP per capita, 2006	.002	Sector share 2	0
RGSP per capita, 2002	.001	Sector share 9	0
Population density	0		
Unemployment rate	0		
Sector share 1	0		
Sector share 3	0		
Sector share 4	0		
Sector share 5	0		

(b) Employment	
Bachelors or higher	.209
Employee compensation per capita	.190
Middle skill workforce	.182
Sector share 1	.118
Population density	.078
Employment, 2001	.047
Employment, 2005	.045
Employment, 2011	.041
Sector share 2	.038
Prime age male	.034
Employment, 2008	.015
Labor force participation rate	.001
Population growth	.001
Employment, 2009	.001
Sector share 7	0
Sector share 9	0
High school or lower education	0

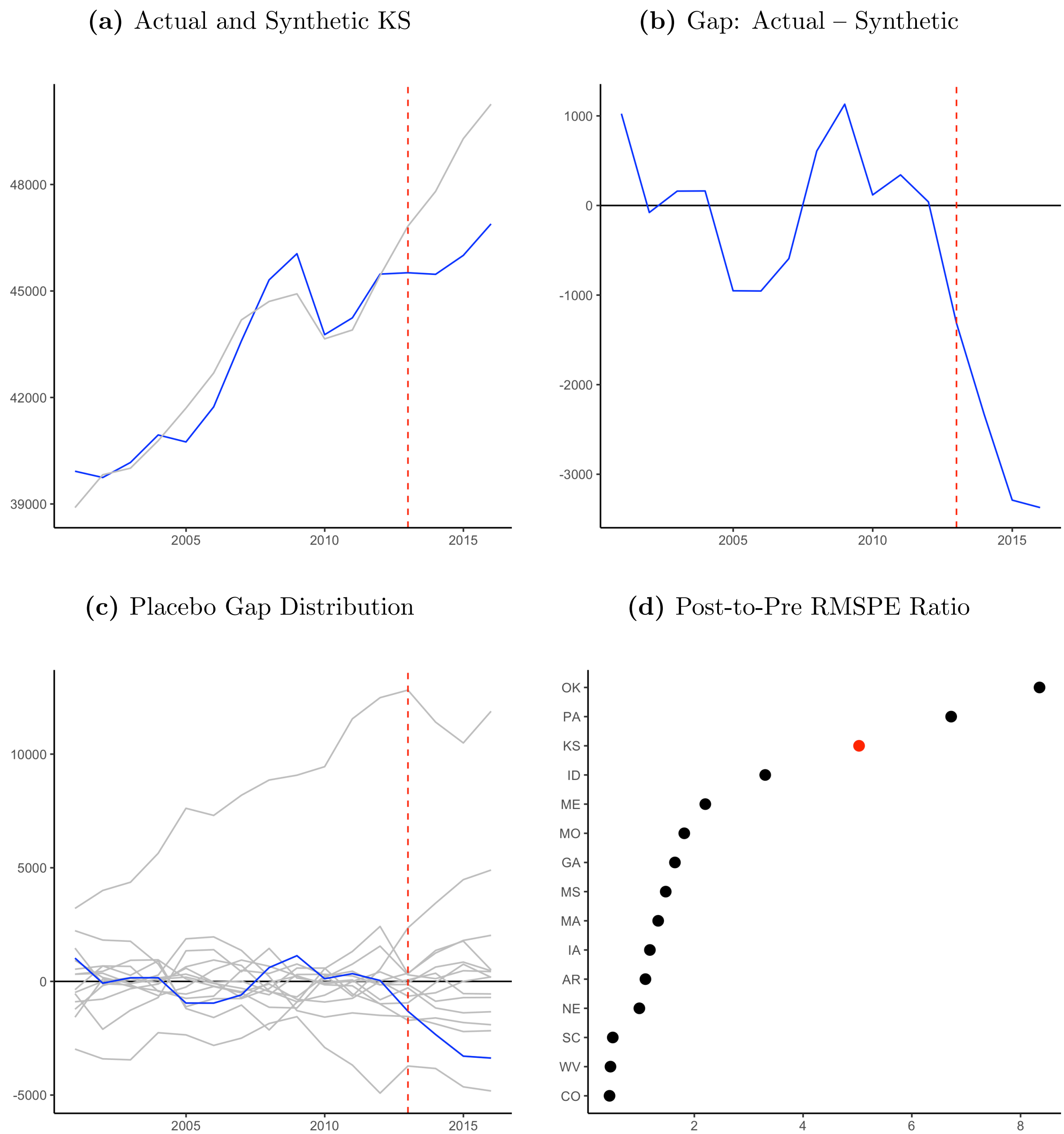
Baseline analysis variable weights. See text for additional detail.

Table 1.3: Predictor Variable Average Values

	Avg	KS	SC_{RGSP}	SC_{Emp}	SC_{Estab}
Pop Growth	0.806	0.573	0.717	0.556	0.499
Population density	0.019	0.03	0.018	0.028	0.023
Prime Male	20.544	20.32	20.41	20.545	20.426
Sector 1	4.16	3.96	8.74	4.29	3.83
Sector 2	5.12	4.32	4.41	4.73	4.83
Sector 3	13.10	15.91	10.96	-	-
Sector 4	18.31	19.30	17.11	-	-
Sector 5	3.77	5.22	3.24	-	-
Sector 6	16.88	14.62	15.81	-	-
Sector 7	9.80	8.55	9.78	10.62	8.75
Sector 9	3.66	3.05	-	3.38	3.43
High School/Lower	47.9	39.8	46.0	43.9	45.9
Bachelor/Higher	26.0	31.1	27.4	29.1	27.2
Middle Skill	45.1	43.7	44.4	43.7	44.3
Unemp Rate	6.0	5.6	5.1	-	-
LFPR	65.1	69.4	64.6	68.4	67.7
Empee Comp	21,470	22,732	-	24,019	22,629
Sales Rate	5.166	5.282	4.755	-	-
RGSP 2001	39,334	39,745	39,823	-	-
RGSP 2002	39,630	40,169	40,009	-	-
RGSP 2003	40,442	40,945	40,782	-	-
RGSP 2006	42,377	43,593	44,186	-	-
RGSP 2007	42,709	45,314	44,705	-	-
RGSP 2008	42,544	46,050	44,920	-	-
Emp 2001	2,498,662	1,767,584	-	1,760,015	-
Emp 2005	2,573,656	1,767,517	-	1,778,566	-
Emp 2008	2,684,307	1,861,559	-	1,851,232	-
Emp 2009	2,612,590	1,818,445	-	1,815,967	-
Emp 2011	2,636,823	1,820,268	-	1,829,061	-
Estab 2001	107,874	74,565	-	-	74,530
Estab 2005	113,044	76,173	-	-	76,017
Estab 2008	114,423	76,096	-	-	76,135
Estab 2009	111,951	74,698	-	-	74,596

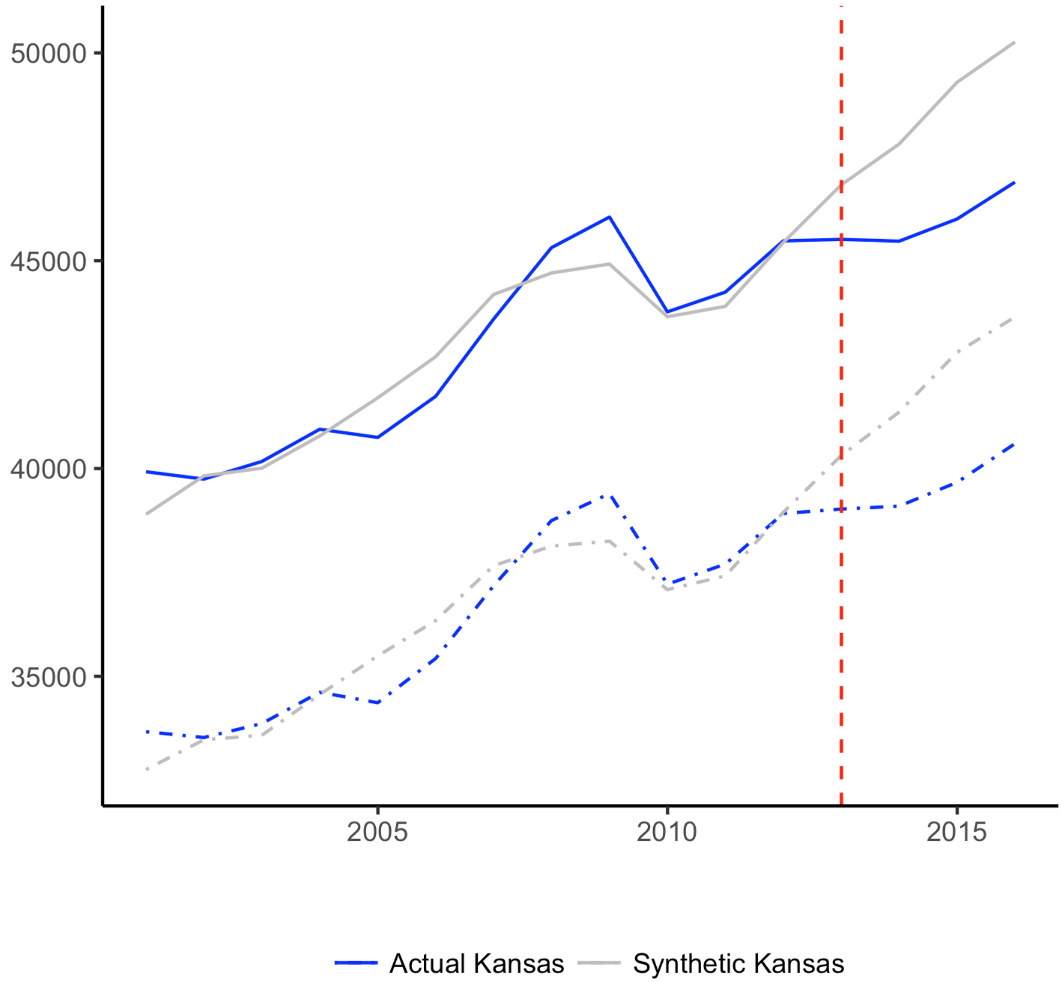
Avg = donor pool mean. SC values are weighted averages using donor pool weights. Weights are different for each outcome. See text for additional detail.

Figure 1.3: SCM Results for Real Gross State Product Per Capita



Dashed red vertical line marks the beginning of 2013 (policy effective date).
(a) Blue = Kansas data. Gray = synthetic control.
(c) Blue = gap between actual and synthetic for Kansas. Gray lines = gap between observed data and placebo synthetic control for each donor pool state.
(d) Post-intervention RMSPE calculated for 2013 to 2015. Pre-intervention RMSPE is calculated for 2001 to 2011.
Source: Annual Gross Domestic Product (GDP) by State, BEA and author calculation.

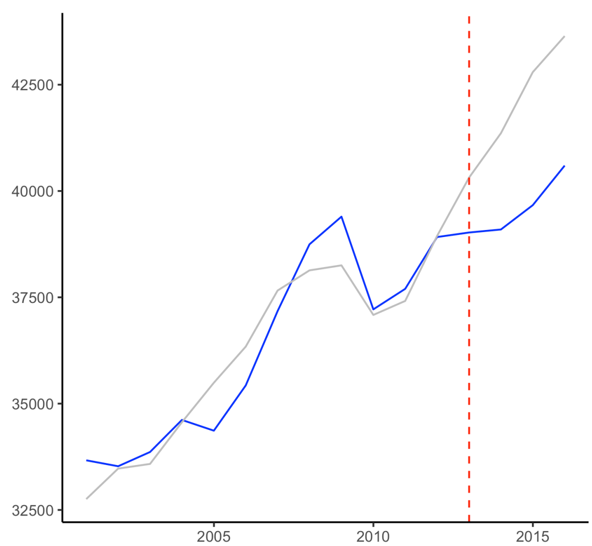
Figure 1.4: SCM Results for Private and Public Components of RGSP



Solid = total RGSP per capita. Dashed = private sector component.

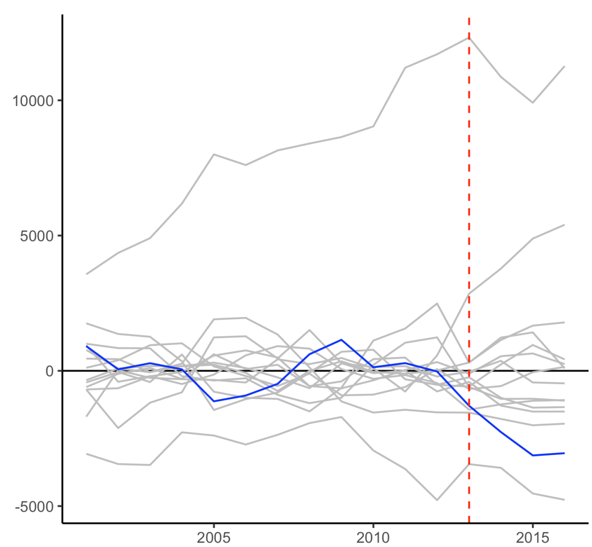
Figure 1.5: SCM Results for Private Sector RGSP Per Capita

(a) Actual and Synthetic KS

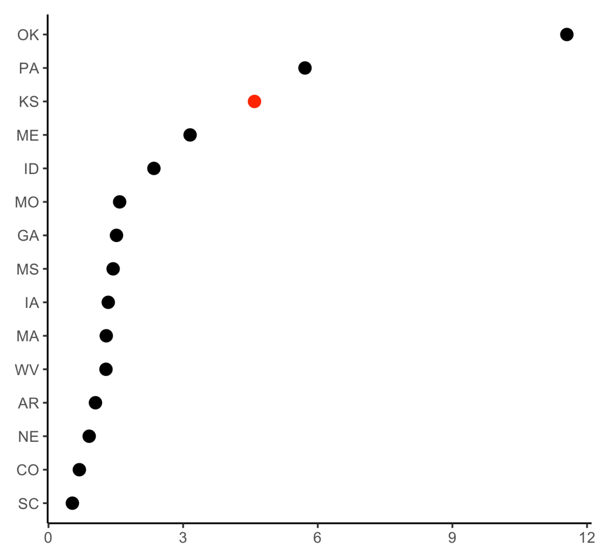


(b) Gap: Actual – Synthetic

(c) Placebo Gap Distribution



(d) Post-to-Pre RMSPE Ratio



Dashed red vertical line marks the beginning of 2013 (policy effective date).
(a) Blue = Kansas data. Gray = synthetic control.
(c) Blue = gap between actual and synthetic for Kansas. Gray lines = gap between observed data and placebo synthetic control for each donor pool state.
(d) Post-intervention RMSPE calculated for 2013 to 2015. Pre-intervention RMSPE is calculated for 2001 to 2011.
Source: Annual Gross Domestic Product (GDP) by State, BEA and author calculation.

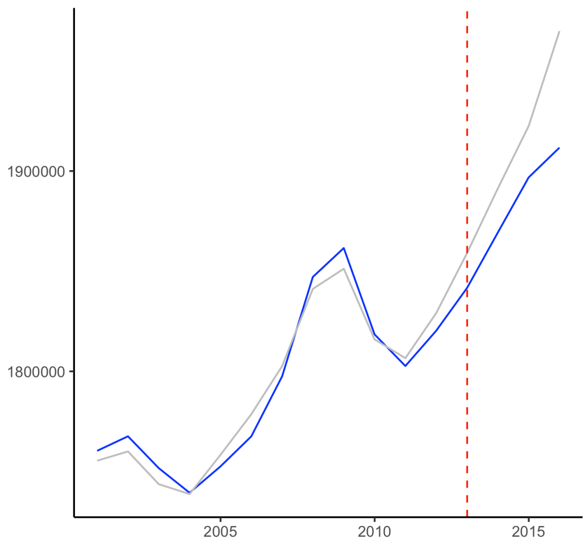
Table 1.4: RGSP Synthetic Control Results

	RGSP	Priv RGSP
Average Treatment Effect	-2,999	-2,813
Dynamic Treatment Effect		
2013	-2,337	-2,264
2014	-3,289	-3,128
2015	-3,372	-3,046
RMSPE		
Pre	604	619
Post	3,036	2,839
Ratio	5.03	4.59
Empirical RMSPE Ratio P-value	.2	.2

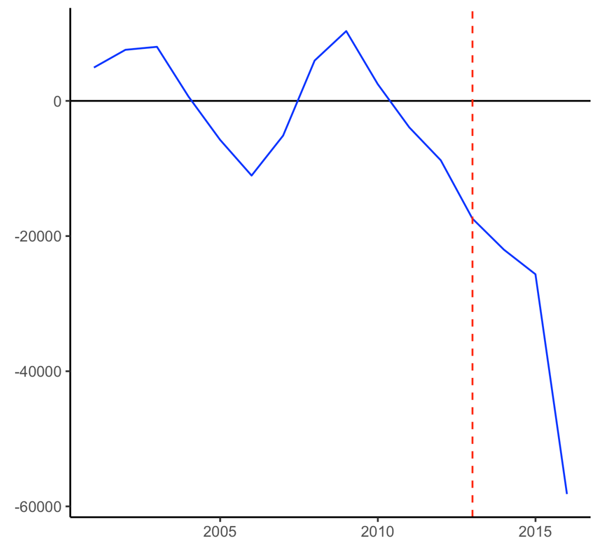
See text for additional detail.

Figure 1.6: SCM Results for Total Employment

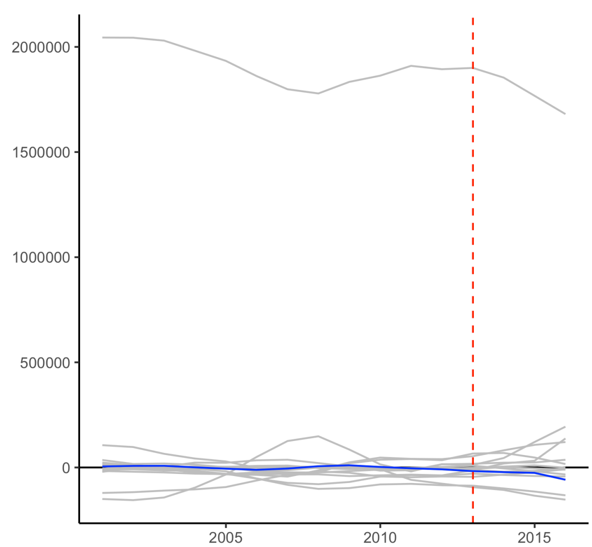
(a) Actual and Synthetic KS



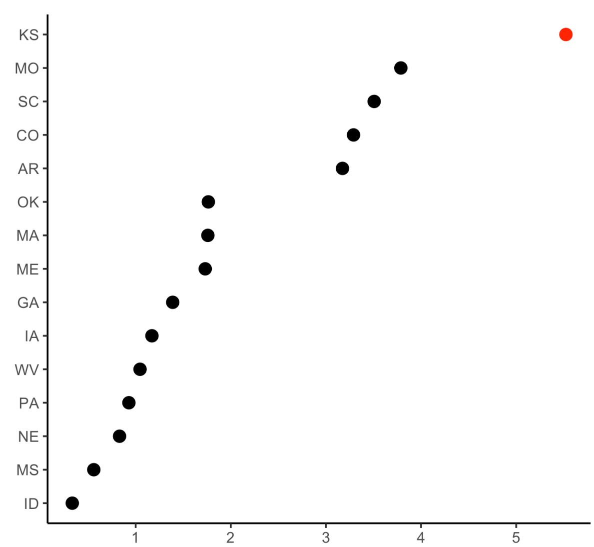
(b) Gap: Actual – Synthetic



(c) Placebo Gap Distribution



(d) Post-to-Pre RMSPE Ratio

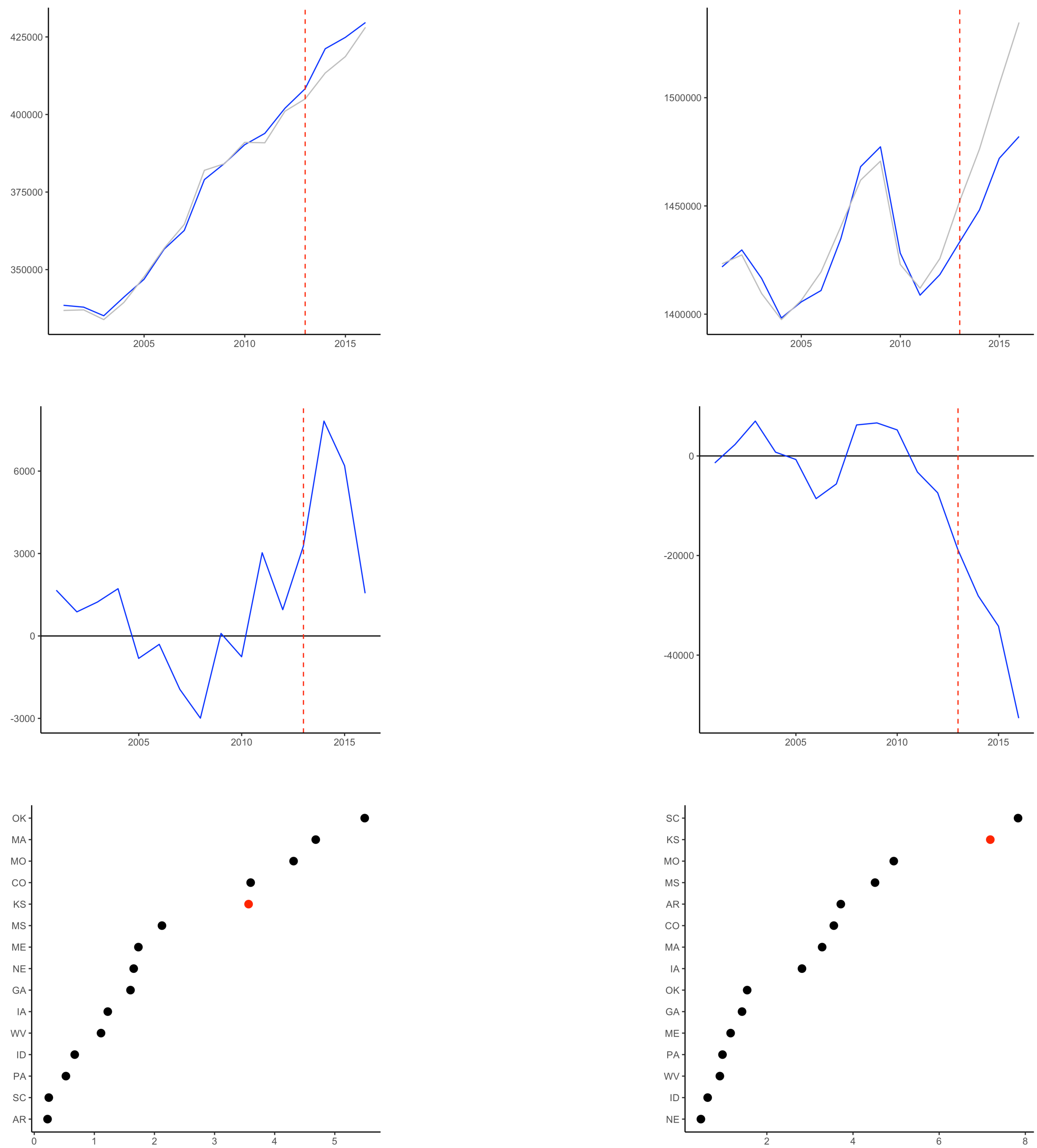


Dashed red vertical line marks the beginning of 2013 (policy effective date).
(a) Blue = Kansas data. Gray = synthetic control.
(c) Blue = gap between actual and synthetic for Kansas. Gray lines = gap between observed data and placebo synthetic control for each donor pool state.
(d) Post-intervention RMSPE calculated for 2013 to 2015. Pre-intervention RMSPE is calculated for 2001 to 2011.
Source: BEA and author calculation.

Figure 1.7: SCM Results for Components of Employment

(a) Proprietor Employment

(b) Wage & Salary Employment



Dashed red vertical line marks the beginning of 2013 (policy effective date). (1) actual and synthetic comparison (blue = Kansas, gray = synthetic Kansas), (2) actual and synthetic gap, and (3) placebo analysis RMSPE ratio distribution. Source: BEA and author calculation.

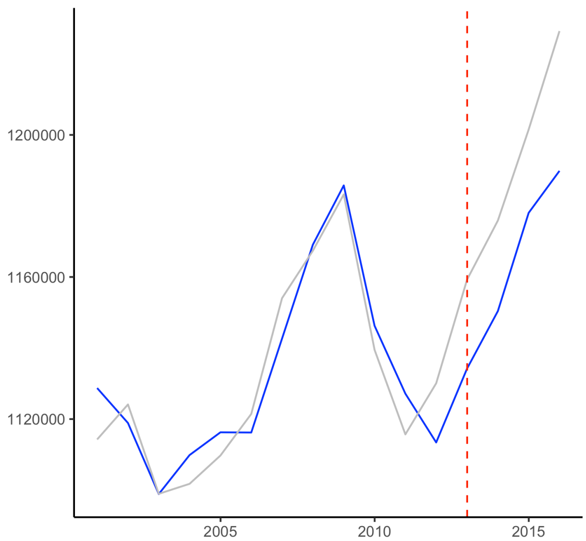
Table 1.5: Employment Synthetic Control Results

	Total	Proprietor	Wage & Salary	CBP
Average Treatment Effect	-35,279	5,191	-38,314	-29,429
Dynamic Treatment Effect				
2013	-22,013	7,820	-28,094	-25,483
2014	-25,649	6,198	-34,186	-23,453
2015	-58,174	1,554	-52,662	-39,349
RMSPE				
Pre	7,034	1,635	5,526	8,289
Post	38,844	5,831	39,713	30,264
Ratio	5.52	3.57	7.19	3.65
Empirical RMSPE Ratio P-value	.067	.333	.133	.133

See text for additional detail.

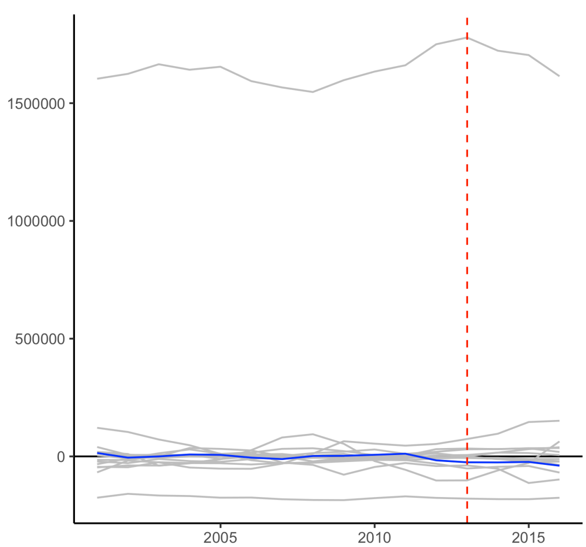
Figure 1.8: SCM Results for CBP Employment

(a) Actual and Synthetic KS

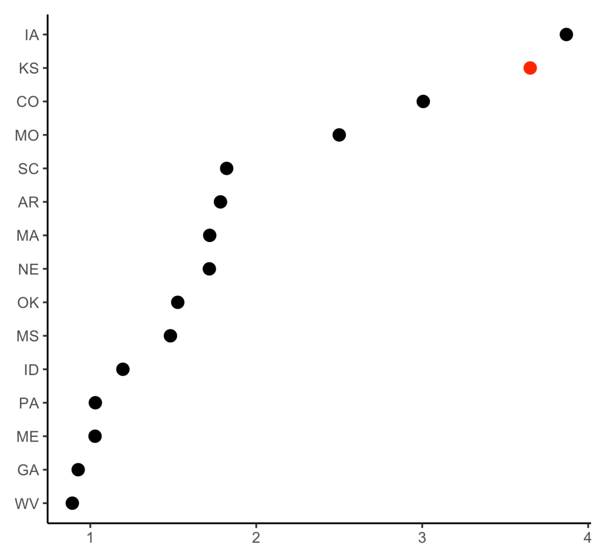


(b) Gap: Actual – Synthetic

(c) Placebo Gap Distribution

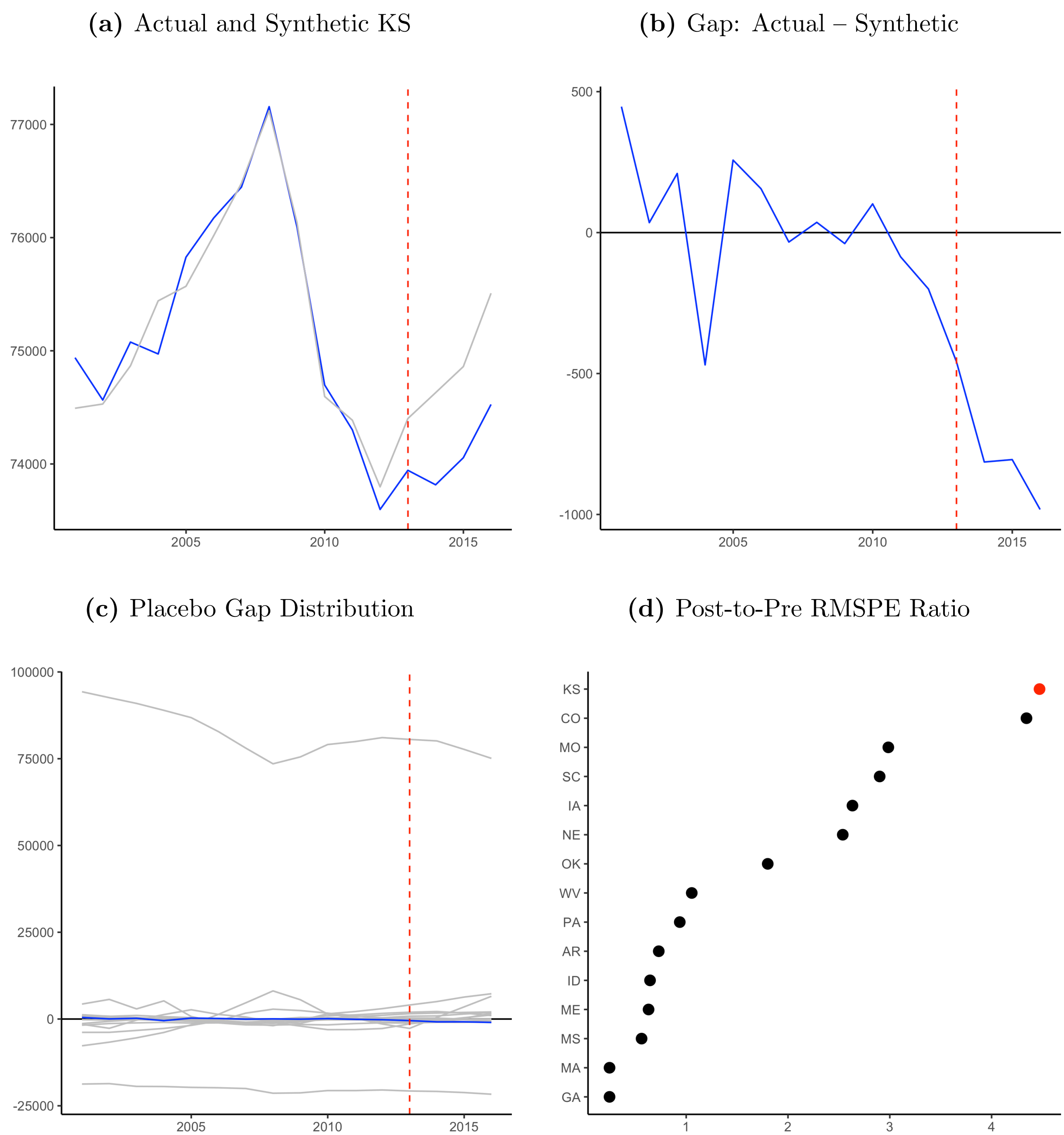


(d) Post-to-Pre RMSPE Ratio



Dashed red vertical line marks the beginning of 2013 (policy effective date).
(a) Blue = Kansas data. Gray = synthetic control.
(c) Blue = gap between actual and synthetic for Kansas. Gray lines = gap between observed data and placebo synthetic control for each donor pool state.
(d) Post-intervention RMSPE calculated for 2013 to 2015. Pre-intervention RMSPE is calculated for 2001 to 2011.
Sources: County Business Patterns, U.S. Census.

Figure 1.9: SCM Results for Statewide Establishments



Dashed red vertical line marks the beginning of 2013 (policy effective date).
(a) Blue = Kansas data. Gray = synthetic control.
(c) Blue = gap between actual and synthetic for Kansas. Gray lines = gap between observed data and placebo synthetic control for each donor pool state.
(d) Post-intervention RMSPE calculated for 2013 to 2015. Pre-intervention RMSPE is calculated for 2001 to 2011.
Source: County Business Patterns, U.S. Census and author calculation.

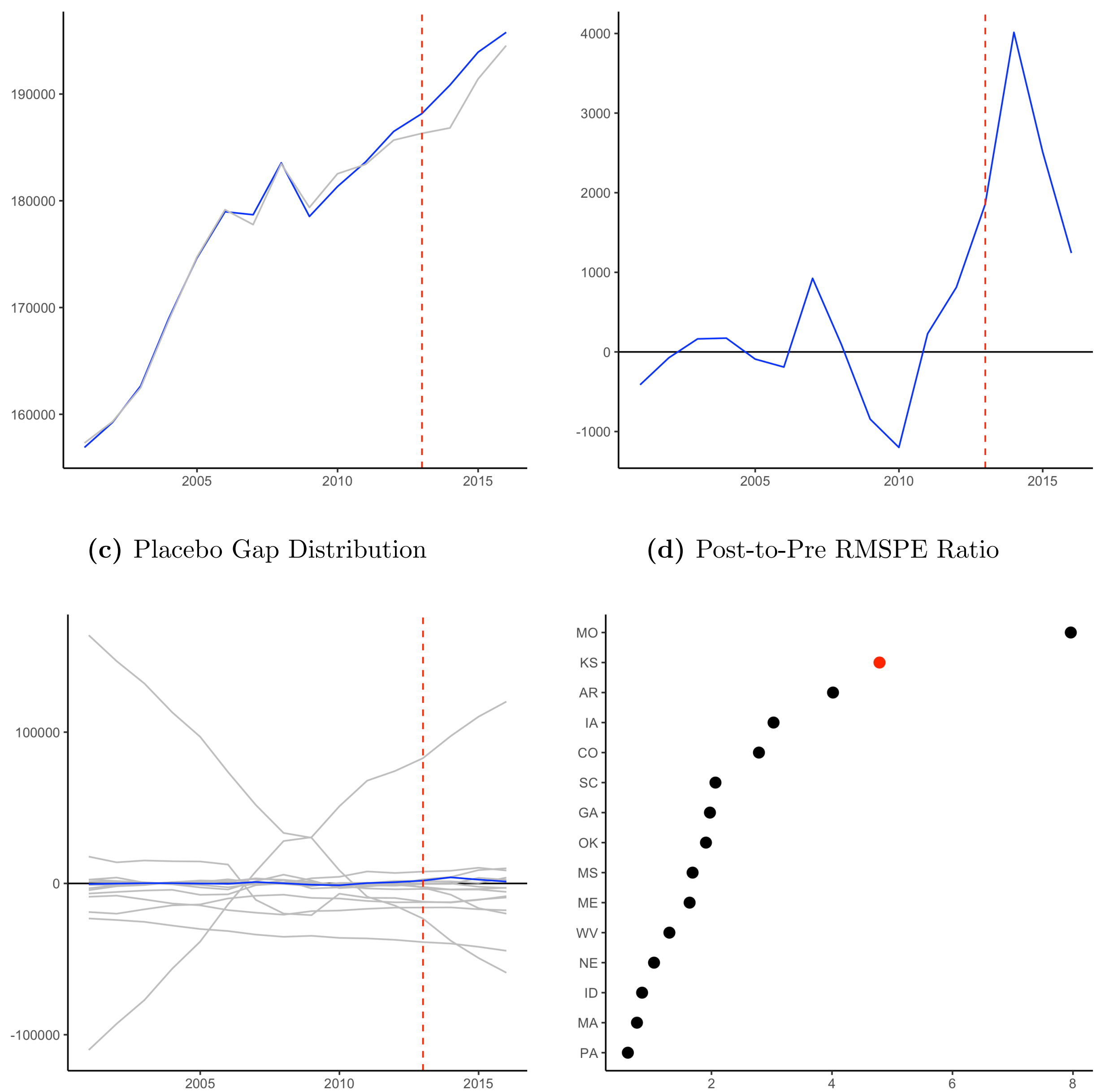
Figure 1.10: SCM Results for Nonemployer Establishments

(a) Actual and Synthetic KS

(b) Gap: Actual – Synthetic

(c) Placebo Gap Distribution

(d) Post-to-Pre RMSPE Ratio



Dashed red vertical line marks the beginning of 2013 (policy effective date).
(a) Blue = Kansas data. Gray = synthetic control.
(c) Blue = gap between actual and synthetic for Kansas. Gray lines = gap between observed data and an estimated placebo synthetic control for each state in the donor pool.
(d) Post-intervention RMSPE calculated for 2013 to 2015. Pre-intervention RMSPE is calculated for 2001 to 2011.
Source: Nonemployer Statistics, U.S. Census.

Table 1.6: Establishments Synthetic Control Results

	Establishments	Nonemployer Estabs
Average Treatment Effect	-867	2,590
Dynamic Treatment Effect		
2013	-814	4,014
2014	-805	2,512
2015	-982	1,243
RMSPE		
Pre	195	590
Post	871	2,827
Ratio	4.47	4.79
Empirical RMSPE Ratio P-value	.067	.133

See text for additional detail.

Table 1.7: Difference-in-Difference Estimates

	RGSP	Emp	Est
$KS \times Post$	-1,216.2	-13,513.38	-3,120.35***
	(1,169.13)	(20,337.59)	(885.90)
$KS \times 2013$	-962.85	-11,779.79	-2,149.94***
	(955.95)	(22,131.35)	(725.32)
$KS \times 2014$	-1,535.58	-7,626.54	-3,075.69***
	(1,306.14)	(22,230.95)	(921.04)
$KS \times 2015$	-1,150.25	-21,133.79	-4,135.44***
	(1,307.21)	(19,149.29)	(1,497.51)
Observations	75	75	75
Average:			
R2	0.0218	0.0061	0.1046
Adjusted R2	-0.3161	-0.3373	-0.2047
F Statistic	1.227	0.3369	6.424**
Dynamic:			
R2	0.0229	0.0074	0.1134
Adjusted R2	-0.3643	-0.3859	-0.2379
F Statistic	0.4135	0.132	2.2601*

Robust standard errors reported in parenthesis. Estimated with state and year fixed effects. Estimated for 2001 to 2015, including state-level observations for IA, KS, MO, NE, OK. Dynamic and average effects estimated separately.

Chapter 2

State Adoption and Implementation of Numeric Nutrient Criteria

2.1 Introduction

The concentration of nutrients (particularly nitrogen and phosphorus) in surface waters currently pose substantial threats to water quality in the United States. These threats have important human health, animal health, and economic consequences. Nutrients in water lead to photosynthesis which leads to plant growth, and particularly to algae growth. Nutrient levels in excess of what the ecosystem can naturally handle cause problematic, excessive plant growth. Plants take oxygen from the water, killing fish, and can make water treatment more cumbersome and expensive. Excessive nutrients can also lead to toxic algae that make human and animal contact with affected water unsafe, and potentially deadly. In recent years, the presence of such toxins has caused beaches to shut down and resulted in water being declared undrinkable. Nutrient over-enrichment is widely accepted as the cause of the hypoxic ('dead') zone in the Gulf of Mexico.

Nutrients are naturally occurring and necessary. Problematic, excessive nutrient presence and its results are described by the phrases 'cultural eutrophication,' 'nutrient over-enrichment,' and 'nutrient pollution.' These phrases are viewed and used here interchangeably. Generally the distinction between nutrient presence of concern and that not of concern lies in connection to human activity. Naturally occurring levels of nutrients are not the concern of policy efforts. Nitrogen (N) and phosphorus (P) are the nutrients primarily viewed as responsible for nutrient pollution. Both enter surface waters from a similar set of sources: municipal and industrial wastewater discharges, agricultural and urban runoff, and atmospheric deposition and automobile exhaust (EPA 1988a; EPA 1994a). Select domestic products, such as detergents, also tend to contain phosphates (EPA 1988b). In some settings only one of the two, N or P, will be the "limiting nutrient," meaning that the non-limiting nutrient is not thought to contribute excessive plant growth. In most freshwater ecosystems, P is the limiting nutrient with respect to plant growth and eutrophication, but in some cases N can be (EPA 1994a).

Nutrient pollution has been a recognized problem in the US since at least as early as the nineteen-sixties.[1] In the nineties the Environmental Protection Agency (EPA) began a new campaign to address the problem. Nationally, under the Clean Water Act (CWA), the EPA is charged with maintaining the nation's water quality. Nutrient pollution falls within that scope. Under the CWA framework, both the EPA and the states play roles in establishing, implementing, and enforcing regulatory policies. In the case of nutrient pollution, EPA developed Guidelines for the states to use in establishing their own nutrient policies. A major aspect of the Guidelines was development of numeric nutrient criteria (NNCs). NNCs would impose quantitative restrictions on nutrient-related parameters. Quantitative restrictions were thought to be easier to implement.

EPA Guidelines for water quality standards are theoretically mandatory instructions to the states. If a state fails to adopt a criterion as recommended by EPA Guidelines, or fails to adopt an acceptable alternative, then the EPA may promulgate federal regulations making its own version of the criterion binding law in the state. The EPA finished developing most of its nutrient-related Guideline documents by the end of 2001, and at that time, instructed states to begin the process of adopting NNCs. By 2016, only twenty-eight states had adopted NNCs that at least partially satisfy the EPA Guidelines. Some states were still working on developing their own NNCs while others preferred alternative approaches. Thus far, EPA

[1] For example, the problem was recognized in an international agreement with Canada during that period.

has only used its power to impose federal NNCs in three cases (one of which was long before 2001).

This chapter seeks to answer two questions: (1) What factors are associated with regulatory approaches taken by states to address nutrient pollution, and in particular associated with a states adoption of an NNC? (2) Are differences in the form of restriction adopted by a state associated with differences in implementation? In terms of implementation, I examine the association between adopting an NNC and imposition of permit discharge limits. This chapter only seeks to answer the question of how NNCs have worked in terms of translating into permit limits. It does not seek to evaluate the overall program or other outcomes of interest. Future work could do this.

Section 2 reviews related literature. Section 3 provides regulatory background. Section 4 describes the conceptual framework underlying the analysis. Section 5 describes the data and econometric framework. Section 6 discusses econometric methods. Section 7 presents the econometric results. Section 8 concludes.

2.2 Literature Review

This study contributes to two main areas of research. The first relates to state approaches to environmental regulation, to which this chapter contributes by examining state approaches to water quality standards. The second relates to implementation of water quality standards through permit limits.

2.2.1 State Approaches to Environmental Regulation

No prior empirical study in economics, to the best of the author's knowledge, directly analyzes different state approaches to water quality standards. Closely related literature analyzes state approaches to environmental regulation decentralization. In the decentralization literature, a number of studies indirectly infer factors related to a state voluntarily assuming control over aspects of its own regulatory program by comparing potentially related outcomes in states that have and have not done so (Sigman 2003; Sjöberg and Xu 2018). In that context a state can choose to establish its own program to replace an EPA directed program. States are given the option of doing this under several federal environmental statutes. States might voluntarily opt to take control over aspects of environmental regulation based on preferences for stringency. That is, a state may prefer more or less stringent regulation than would be provided by the EPA. Empirical results on this point are mixed. Sigman (2003) finds some evidence that states decentralize to increase the stringency of environmental regulation. Sjöberg and Xu (2018) find that enforcement decentralization under RCRA is not associated with significant changes in enforcement behaviors (inspection frequency, number of detected violations, the amount of monetary penalties, and enforcement efficiency). This suggests that stringency, in terms of enforcement, was not associated with state action on decentralization.

Assuming program responsibility and adopting NNCs differ in that the former is completely voluntary. States have no mandatory obligation to assume responsibility and face no potential recourse for failing to. In the NNC water quality context, states are legally obligated to comply with EPA Guidance, and should they fail to, they face the prospect of federally imposed regulations.

More similar to the present study Ramirez Harrington (2013) analyzes the relationship between state adoption of environmental policies and potentially influential factors, although not in the water quality context. She finds that prior firm-level environmental performance and prior state-level enforcement history are not significant determinants of state policy adoption.

2.2.2 Implementing Water Quality Standards Through Permit Limits

Few studies analyze permit limits as an outcome, although many examine how they relate to other outcomes, such as inspections and enforcement. Mickwitz (2003) comes closest to considering the question considered here, although does so in a different regulatory setting: Finland. Analyzing why some among a group of pulp and paper facilities are assigned discharge limits for phosphorus, while others are not, he finds evidence that prior-year discharges increase the probability of having a limit imposed, and that discharging into the Baltic Sea (which is viewed as away from the land) decreases the probability. Other factors examined had no systematic effect.

As discussed further in the next section, in the US considerations for setting permit limits are set by law, and should apply similarly to similarly situated facilities. However, there is some evidence that this may not always occur. Earnhart and Glicksman (2011) find substantial variation in discharge limits imposed across wastewater discharging facilities and over time, even within a single industry and for a relatively comparable set of facilities (major facilities). DeShazo and Lerner (2004) find evidence of politically motivated discrimination based on firm size in setting discharge limit stringency. In particular, larger firms were found able to secure less stringent limits for their plants, but the firm size effect varied greatly depending on local interest group influence. They analyze BOD and TSS limits imposed on facilities in the pulp and paper industry.

2.2.3 Contributions of this Study

This study has three major contributions. First, it provides evidence related to the nutrient context otherwise lacking in the extant literature. Nutrient pollution has been a top environmental regulatory priority for over a decade, but progress has been slow (Mississippi River Collaborative 2016). Evaluating the effectiveness of the EPA approach relative to others, and the factors associated with a state-level approach provides valuable information for policymakers. Second, it directly characterizes factors associated with state policy adoption. Much prior work in this area only makes indirect inferences regarding such factors. Third, it provides evidence on how adopted standards relate to the imposition of permit limits. There is evidence that once imposed, firm compliance with effluent discharge limits written into permits is generally strong. Earnhart and Glicksman (2011) find near universal compliance with discharge limits, and strong over-compliance with those limits a majority of the time, suggesting that tightening discharge limits is generally an effective means of lowering discharges, or that currently imposed limits are not sufficiently stringent to be binding. The contrast between, on the one hand, high compliance with imposed limits, and on the other hand, excessive pollution and perceptions of lax control, suggests a need for further examination of limit setting.

2.3 Regulatory Context

This section describes state water quality standards (which are the set of rules at the state-level that an NNC would be in), and how they are implemented, including their relationship to discharge permit limits. It also provides additional background specific to the nutrient context.

2.3.1 Water Quality Standards

The CWA and EPA require states to adopt water quality standards to ensure minimally adequate water quality levels. Water quality standards place restrictions on the ambient levels of pollutants that can be in waters (Earnhart and Glicksman 2011). The specific restrictions are referred to as "criteria." Criteria are defined as "... elements of State water quality standards, expressed as constituent concentrations, levels, or narrative statements, representing a quality of water that supports a particular use" (40 CFR 131.3(b)).

Under the CWA, EPA is required to develop and provide Guidance to assist states in developing water quality standards. EPA Guidance contains recommended criteria based on the latest scientific information about how pollutants affect aquatic species and human health. States are required to use that Guidance, to develop their own criteria, which are then included in their water quality standards. States can set the criteria values based on EPA Guidance, on a modified version of that Guidance driven by local conditions, or on another scientifically defensible method. States are also free to enact more protective water quality standards than required by the EPA.

State water quality standards and criteria must be approved by the EPA. They must also be periodically reviewed and updated to reflect current scientific standards. If a state fails to adopt a satisfactory criterion, the EPA can promulgate one for the state in the form of a federal regulation. Once state adopted criteria are approved by the EPA (or federally imposed criteria become final regulations) they become binding. Once binding, criteria are implemented through permit limits, assessment and impairment designations, and through Total Maximum Daily Loads.

2.3.2 Impaired Waters and TMDLs

Impaired water designations and Total Maximum Daily Loads (TMDLs) are two ways water quality standards and criteria are implemented. If waters fail to satisfy applicable criteria, those waters are considered 'impaired.' Since 1992, EPA has required states to establish impaired waters lists every two years (CRS 2012). Once a waterbody is designated as impaired, the state is required to establish a Total Maximum Daily Loads (TMDLs). This is a number which represents the maximum amount of the pollutant that can be present in the water while still allowing water quality standards to be attained. That amount is allocated among sources of pollution so that aggregate discharges from all sources do not exceed the TMDL amount (Earnhart and Glicksman 2011, 40-41). EPA reviews and approves state impaired waters lists and TMDLs. A water body typically remains on the impaired waters list until it has a EPA approved TMDL. EPA sets a goal of attaining water quality standard criteria within eight to thirteen years of a water being listed as impaired (CRS 2012, 3). EPA is required to develop impaired waters lists and make TMDL determinations for states that do not do so on their own.

One of EPA's reasons for promoting NNCs is that numeric criteria should facilitate impairment designations and establishment of TMDLs. NNCs should facilitate impairment designations because the determination could be made based on a single measured value. NNCs would facilitate TMDL development and implementation by providing a set numerical goal to be attained. More recently EPA has encouraged states to designate waters as nutrient impaired even where numeric criteria are not in place and offered examples of ways other states have justified doing so (EPA 2013b). Multi-jurisdiction TMDLs are also now being used to address nutrient-related impairment. In 2010 EPA issued a multi-jurisdiction nutrient-related TMDL for the Chesapeake Bay. At the time it was the largest TMDL ever undertaken. The TMDL allocates needed reductions to seven different jurisdictions: New York, Pennsylvania, Maryland, Delaware, Virginia, West Virginia, and DC. EPA's goal is to achieve all required reductions by 2025. EPA put 'backstop' measures in place at the outset to ensure targets will be achieved and has threatened to do so again should progress be found unsatisfactory. Nutrient trading programs are also being supported in the area (CRS 2012, 12-13). New York and Connecticut also have a multi-jurisdiction TMDL to address nutrient-related impairment in the Long Island Sound. As discussed further below, some states prefer to deal with nutrient pollution using TMDLs rather than NNCs.[2]

2.3.3 NPDES Permits and Polluting Facility Discharge Limits

The other avenue for implementation of water quality standard criteria, and the aspect analyzed in this chapter, is through permit limits. The CWA makes discharge of pollutants into

[2] TMDLs are not necessarily advantageous to states and there are cases where states have opposed establishing a nutrient-related TMDL.

the nation's surface waters illegal unless done and in accordance with a valid permit. The National Pollutant Discharge Elimination System (NPDES) permit program covers discharges of industrial and municipal effluents into surface water bodies (Earnhart and Glicksman 2011, 33). These permits set 'discharge limits' constraining either quantities or concentrations of particular pollutants that the permit holder is authorized to discharge into water at a particular point. NPDES permits apply to and regulate 'point source pollution.' The term 'point source' is defined as: "any discernible, confined and discrete conveyance, including but not limited to any pipe, ditch, channel, tunnel, conduit, well, discrete fissure, container, rolling stock, concentrated animal feeding operation, or vessel or other floating craft, from which pollutants are or may be discharged" (CWA § 502(14)). Anything not covered by that definition is a nonpoint source, and is not subject to federal regulation under the CWA.[3]

Individual facilities who are point-source polluters (or aspiring polluters) apply for NPDES permits. Permits contain limits for each source of discharge and each pollutant to be discharged. Permits are effective for a fixed term of at most five years. They can be modified, revoked, or terminated for cause (Earnhart and Glicksman 2011, 39-40). Permits are issued by the EPA or by an authorized state agency. If a state has established a permit program, with EPA approval, then the designated state agency has primary responsibility for permit administration. EPA retains veto power over individual state permits. If a state chooses not to maintain an approved permitting program, EPA will handle permitting for the state. The state and EPA share concurrent enforcement authority over issued permits (Earnhart and Glicksman 2011, 34-35).

Broadly there are two types of permit limits, technology-based and water quality-based. At the federal level, EPA sets technology-based "effluent limitations" to be put into permits by permit writers. Federal effluent limitations are industry specific. States may adopt their

[3] However, federal provision may in some circumstances require that a state take action to limit nonpoint polluter discharges, such as, for example, where a TMDL is in place (because the TMDL limit would include aggregate discharges from all sources).

own effluent limitations, though are not required to.[4] Federal effluent limitations are updated periodically to reflect the current levels of pollution control deemed to be technologically and economically achievable based on currently existing technology. Updating of federal effluent limitations is one reason that otherwise similarly situated facilities might have different technology-based permit limits. Another potential source of facility-based difference are provisions subjecting newer facilities to more stringent effluent limitations. Variation in state rules related to interpretation and application of limits can also result in permit limit variation, even when the same underlying effluent limitation is used as the reference in writing those limits (Earnhart and Glicksman 2011). If no effluent limitation exists for a particular pollutant, then permit writers are to set facility-specific limits based on their own best professional judgment. The best professional judgment limit should take into account the same factors considered in establishing effluent limitations (e.g., technology-based).

Permits can also include water quality-based discharge limits. Water quality-based limits are derived from state water quality standards. These can vary based on conditions and characteristics of the receiving waterbody, just as water quality standards do. Water quality-based limits are derived to ensure water quality standards are satisfied (typically meaning that the criteria contained in the water quality standards will not be exceeded). Unlike effluent limitation based permit limits, water quality-based limits are set without reference to technology or feasibility. If a particular pollutant is covered by both a technology-based effluent limitation and a water quality-based limitation, the resulting permit limit is set as the more stringent (pollutant minimizing) of the two. If a TMDL is in effect for the receiving waterbody, a permit limit may also incorporate a TMDL required discharge reduction.

Facilities can attempt to avoid or reduce a permit limit by obtaining a variance, or by participating in a trading program. Both variances and trading programs are important in

[4] In a case of conflicting state and federal effluent limitations, the more restrictive would apply – meaning states cannot set their own effluent limitations to avoid more restrictive federal ones.

the nutrient context, but are beyond the scope of this paper.

Sources of nutrient pollution include both point source dischargers (PSDs) and non-point sources (NPSs). N and P enter waters, from agricultural operations (from livestock operations and row crop runoff), urban and suburban storm-water runoff, municipal wastewater treatment systems/discharges, and air deposition (State–EPA 2009, 22). Many agricultural sources are NPSs, and thus not subject to permit based restrictions. Concentrated Animal Feeding Operations (CAFOs) are an exception, subject to NPDES regulation. Urban stormwater in areas covered in Municipal Separate Storm Sewer Systems and Combined Storm Sewer Systems are subject to NPDES permit limits. Communities and growth outside these areas are only subject to construction storm water general permits. Municipal wastewater treatment facilities are subject to discharge limits, but as of 2009, only a small subset had numeric N or P limits written into their permits (approximately 4.4 percent had numeric limits for N, and 9.9 percent for P). On-site and decentralized wastewater treatment systems fall outside the scope of the NPDES permit system (State–EPA 2009).

2.3.4 Federal Guidance for Nutrients

In 1998, EPA made nutrient overenrichment a top national environmental priority (EPA 1998a; EPA 1998b). It established a plan to develop a set of numeric criteria Guidelines. The criteria would include different recommended values for each of four different waterbody types, in each of fourteen different ecoregions, and for each of four different parameters. The four waterbody types are: lakes, rivers, estuaries, and wetlands. Not all states have all four, but all have at least two. Ecoregion boundaries do not follow jurisdictional borders, and most, if not all, states contain more than one of the fourteen ecoregions. The four parameters are: total nitrogen, total phosphorus, chlorophyll-A, and turbidity. This 'menu of different values approach,' offering at least 56 options, contrasts the more common approach to regulating water pollutants, which can typically be done with reference to a single value (EPA 1998b, iv). Guidelines for three waterbody types were released by the end of 2001. At that point

EPA directed states to submit Nutrient Criteria Development Plans (NCDPs) "to outline their process for how and when they intend to adopt nutrient criteria into their water quality standards" (EPA 2001a).

As of 2018, at least forty-six states have submitted plans. Fewer have adopted NNCs. Every few years, the EPA makes a renewed declaration of interest in nutrient criteria development. Table 2.1 provides a time line of federal-level policy statements and guidelines that have come from the EPA in relation to its nutrient policy. EPA has the authority to impose NNCs on states not adopting them on their own, but has made only limited use of that authority. EPA has promulgated nutrient criteria in at least two states, and recently started the process in another. In 1976 it promulgated NNCs for Arizona. They where withdrawn in 2003 when Arizona adopted its own criteria. In 2012 EPA promulgated numeric phosphorus criteria for the Florida everglades. In 2017 EPA proposed NNCs for Missouri, which are still pending.

2.3.5 State Adoption of NNCs and Other Approaches

States have been slow to adopt the type of numeric restrictions recommended by EPA and some have outright refused to do so. A 2009 Inspector General Report found that the EPA needed to accelerate the adoption of NNCs (EPA 2009). Around the same time, a report from the State–EPA Nutrient Innovations Task Group called into question the usefulness of NNCs (State–EPA 2009).

Regulatory efforts are complicated by the fact that naturally occurring nutrient levels vary substantially across time and space, and are a function of many variables. Restrictions can target the consequences of nutrient pollution, such as excessive plant growth or loss of clarity in water. They can also target the pollutants that cause those consequences, nitrogen (N) and phosphorus (P). In either case, the line between natural and unnatural can be challenging to identify. Further complicating the matter, the level of nutrient presence in waters that will lead to the undesirable consequences of nutrient pollution can also vary substantially. EPA Guidelines instruct states to adopt criteria for four parameters, two causal and two responsive. Developing those four criteria for each of the possible waterbody-ecoregion combinations within a state is a large project. The need for customization likely contributes to the lack of success of a numeric approach in this context.

EPA argues that numeric criteria are needed for effectiveness, and specifically to be translated into permit limits, TMDLs, and TMDL-based permit limits. Additional considerations include the need to protect downstream waterbodys and desire to identify nutrient problems before ecosystem responses are observed. EPA has also argued that NNCs are more efficient than site-specific implementation of narrative standards. However, NNCs are difficult to develop and some states are hesitant to adopt them once developed (e.g., Montana). Some states argue that numeric criteria are unnecessary. Some prefer to take a TMDL-based approach (e.g., Delaware, Ohio). Rather than developing an NNC, which can be used as a basis for classifying waters as impaired and then developing a TMDL, these states prefer to classify waters as impaired and develop TMDLs (presumably relying on their narrative criteria for this purpose). Both NNCs and TMDLs should lead to permit limits. If NNCs lead to more or faster TMDL development, all else constant NNCs would increase (or leave unchanged) their impact on permit limits.

One aspect of EPA's NNC approach to which states have objected is that it calls for setting binding quantitative restrictions on causal parameters without reference to responsive parameters. Opponents of this approach argue that its single-indicator nature is too restrictive, in that the criteria could be exceeded even in cases where the water is not impaired. On this point, EPA notes that the criteria are meant to be protective of far field and downstream waters. Similarly, while nitrogen may not be the limiting nutrient in a particular waterbody, it could become important upon reaching downstream waters. Additionally, the limiting nutrient for a particular waterbody can vary over time as conditions change, both across years and within years seasonally.

2.4 Conceptual Framework

States have taken different approaches to addressing nutrient pollution. The ultimate question of interest here is whether EPA's NNC approach is associated with greater implementation, where implementation is defined in terms of permit limits.[5] Because the approach selected by a state may not be independent of its imposition of permit limits, the analysis first explores factors associated with whether or not a state has chosen to adopt an NNC approach. The remainder of this section describes the conceptual frameworks guiding the empirical analysis.

2.4.1 State Adoption of Numeric Nutrient Criteria

Decision to adopt an NNC could be guided by a number of factors. This paper focuses on three categories of factors and characteristics that may influence a state's approach: (1a) the extent and sources of the problem within the state (water quality, pollution source, and cost related concerns); (1b) preferences and political climate; and (1c) state characteristics (social, economic, demographic). Potentially important factors that are not addressed include ecological diversity, resource availability, including issue specific special funding sources, as well as upstream and downstream considerations. The following a priori expectations apply.

With respect to the extent of the problem, as it worsens, a state may be more likely to take action. As a result, the state may be more likely to adopt an NNC. On the other hand, the extent of the problem could also make it more difficult and cost prohibitive to address the problem. The extent of the problem could also reflect a lack of willingness to or interest in addressing the problem.

Competing water quality concerns may also play a role. As other sources of water quality

[5] As indicated in the prior section, implementation can be broadly viewed as having three aspects. Permit limits are one of those aspects.

problems increase, a state may be less likely to take action to address nutrients. States and environmental departments are resource limited. Developing NNCs requires substantial resources.[6] States with more non-nutrient water quality concerns may devote fewer resources towards nutrients, and, as a result, may be less likely to adopt an NNC. General water quality issues could also signal a lack of concern for environmental protection.

Sources of the problem may influence both the extent of the problem and the likelihood of selecting an NNC approach to address the problem. More generally, larger relative contributions to the problem from nonpoint source contributers may decrease the likelihood that a state adopts an NNC. Greater agricultural activity is expected to increase nutrient pollution, in turn increasing the extent of the problem. Agricultural sources are generally not subject to regulation under the permit program, so they would not be restricted by an NNC implemented through permit limits. Thus, as agricultural activity rises, an NNC would place a greater burden on direct point sources while a larger share of the problem would come from non-point sources. This in turn might make a state less likely to adopt an NNC. An exception are CAFOs, which are subject to regulation under the permit program. Greater livestock activity is expected to increase the extent of the problem, but, in the case of CAFOs, could more effectively be addressed by an NNC. As a result, a state with greater livestock activity may be more likely to adopt an NNC.

With respect to the second category, as others have pointed out, political factors may be related to environmental outcomes and policies (Sjöberg and Xu 2018; Ramirez Harrington 2013; Helland 1998). As the political climate shifts more towards the Republican party, a state is expected to be less likely to adopt an NNC because that political party cares less about environmental protection.

With respect to the third category, holding all else constant, various social, economic, and demographic characteristics of a state might be related to a state's approach. The

[6] See state NCDPs wherein states describe the steps involved in developing NNCs.

economic climate can affect both available resources and the priority given to environmental protection. Social and demographic characteristics are potentially correlated with individual and community preferences and demand for environmental protection.

2.4.2 Implementation of Approaches

Implementation, in the form of permit limits, is potentially related to: (2a) the state approach selected (EPA hypothesis); (2b) legal factors that permit writers are required to consider, date of initial permit, industry technology, and the pollutants a facility will discharge; and (2c) possibly other factors, which technically are not supposed to be taken into account.

2.5 Data

This section describes the variables used in the empirical analysis and the sources of data exploited to generate these variables.

2.5.1 State Nutrient Criterion Policy Data

This study compiles state-level data on nutrient criteria from multiple sources. The analysis focuses on 2004 to 2014. States with NNCs are identified from the EPA website. EPA keeps track of which states have adopted approved NNCs and when they were adopted on its State Progress Toward Developing Numeric Nutrient Water Quality Criteria webpage (Progress dashboard). Any state indicated as having a full or partial NNC are considered here to have at least partially adopted the EPA's NNC approach. The EPA Progress dashboard distinguishes between states with NNCs for N or P and states with NNCs for chlorophyllA (one of the two response parameters for which EPA recommends states adopt criteria). State status is reported as of 1998, 2008 and 2013 for N and P criteria, and only currently for chlorophyll-A criteria. Where possible, more specific years of adoption are obtained from supplemental sources – namely from state NCDP and WQS documents, which are all available from the EPA Progress dashboard.

Table 2.2 provides a timeline of state adoption. Prior to 2004, 13 states had adopted an NNCs for N and/or P. During the sample period, 2004 to 2014, 10 more states adopt NNCs for N and/or P. Two states without N/P NNCs are identified as having adopted a chlorophyll A criterion before 2004. Another two adopted one during the sample period. The analysis only considers first time adoption. No distinction is made for states who continue to develop and adopt more NNCs in years following. Because of the extensive amount of work and time that goes into developing NNCs, states will have likely made the decision to adopt an NNC years before they actually do so. Moreover, before NNCs become binding they are reviewed by the EPA and possibly at the state level. In adopting NNCs, states may be responding to contemporaneous factors or to lagged factors.

2.5.2 Permit Limit and Facility Level Data

Permit, limit, and facility data are obtained from the EPA ICIS-NPDES database. Permit limits are aggregated to the permit-limit effective year level, and then to the state-year level. Permits generally correspond to facilities. Each permit can contain multiple limits. A permit connected to any limit that begins in a given year is treated as a single observation for that year. If any of the permit's limits beginning in the year are N limits, it is treated as containing an N limit. Similarly, if any of the permit's limits beginning in the year are P limits, it is treated as containing a P limit. N and P limits are identified using lists of N and P related parameter codes obtained from the EPA Pollutant Loading Tool Technical Guidance Manual and the Facilities Likely to Discharge technical documentation.[7] Ammonia parameter codes

[7] A parameter code identifies the particular pollutant (by name and form) to which a limit applies. The limits data use over 2,000 unique parameter codes. The two sources combined identify 40 parameter codes associated with nitrogen and 26 parameter codes associated with phosphorus.

are excluded.

The sample of facilities is restricted to those likely to discharge N or P.[8] These facilities are identified using a procedure similar to that described in EPA (2013a). The primary difference is that I do not rely on the condition that a firm has N or P discharges. Thus classification is based primarily on industry codes.[9] Permit writers consider industry in determining which limits to impose, thus this classification approach may be similar to that used in determining which parameters to include in limits when writing permits. Industry-based facility sample restrictions are common in the literature (Earnhart and Rassier 2016; Earnhart 2009). While permits generally last for up to five years, the limits imposed on some facilities observed have start dates suggesting more frequent variation. States have the discretion to require facilities to obtain new permits more frequently than the default five years. States also have the discretion to modify and impose new limits on facilities during a permit cycle. Any year in which a facility obtains new limits is counted as an observation.

The sample consists of 62,035 unique facilities classified as likely to discharge N or P, corresponding to 136,814 facility-year observations. Table 2.3 summarizes the number of facility years with N and P limits and monitoring requirements.

2.5.3 Water Quality

Water quality is difficult to measure. This chapter constructs a measure using EPA data on impaired waterbodies. In particular, this study uses assessed waters data that is reported in terms of two-year cycles. The waterbody level data is aggregated to the state-cycle level. Both counts of impaired waterbodies and cardinal measures are computed. However, because waterbody size is measured in different units, the cardinal analysis measures are scaled using total state water area (in square miles). Variables are constructed for both

[8] Prior to imposing this restriction, the sample contained 79,484 unique facilities (based on NPDES permit ID). The restriction reduces the sample to 62,035 unique facilities.

[9] Both SIC and NAICS codes are used as available.

total impaired waterbodies and waterbodies with nutrient-related impairments. A number of states are missing data for one or more cycle. For cycles lacking data, I impute the average state value from cycles with non-missing data. An indicator is used to control for this imputation and assess its impact on the results. In order to measure water quality in the period leading up to a state's possible adoption of an NNC, I use a lagged measure of water quality (in all specifications).[10] Table 2.4 reports summary statistics for the water quality measures. The waterbody types indicated, lakes, rivers, and estuaries, correspond to the units of measurement, acres, miles, and square miles.

2.5.4 Independent and Control Variables

Potentially important agricultural factors are captured by fertilizer expenditures, feed expenditures, and livestock revenue. These variables should capture variation in the contribution of agricultural non-point sources to nutrient pollution. Personal consumption expenditures on recreational goods and services as a percent of total personal consumption expenditures are used to capture the desire for improved water quality. Control variables are included for economic, social, and demographic characteristics. Economic characteristics include the unemployment rate, per-capita personal income, and industrial sector shares of economic activity (real gross state product). Social and demographic characteristics include age, gender, and education.

Political variables include state party affiliations and League of Conservation Voters (LCV) scores. Governor party affiliation is expressed as democrat, republican, or non-major party. State senate and house affiliations are expressed as the total membership affiliated with each of major parties. The LCV score combines scores for each state's federal congressional members. LCV scores range from 0 to 100, representing the "proportion of time

[10] Because impairment data are available on a two-year cycle, the one year lagged value equals the two year lagged value.

a legislator voted with the LCV's position on selected measures that are considered pro-environment." The value can be interpreted as representing median voter preferences or as a proxy for demand for environmental amenities (DeShazo and Lerner 2004; Sjöberg and Xu 2018).

Summary statistics for all states from 2004 to 2014 are reported in Table 2.5. The data combines a number of sources. Unemployment data are from the BLS. Agricultural factors, personal consumption expenditure, per-capita personal income, and sector share data are from the BEA. Population, gender, and age breakdowns, along with land and water area data are from the Census. Education is from the Current Population Survey. All variables are at (or aggregated to) the state-year level. State governor and legislator party affiliations are compiled from Klarner (2013) through 2011, and from the National Conference of State Legislatures State Partisan Composition Data following 2011.

2.6 Econometric Methods

Adoption of an NNC is a modeled as a binary choice. Thus ordinary least squares is not appropriate. I use a probit model, where $Y_{s,t}$ equals 1 if state s has adopted an NNC in year t. The empirical model is:

$$\Pr(\mathrm{Y_{s,t}} = 1|\mathrm{X_{s,t}}) = \Phi(\beta_0 + \beta_1 \mathrm{X_{s,t}}),$$

where Φ is the cumulative standard normal distribution function and $X_{s,t}$ is a vector of factors potentially related to adoption of an NNC and state characteristics for state s at time t. Fixed effects are not included in estimating the probit model (Greene 2002). States are kept in the sample after adopting an NNC. Theoretically, a state could modify or reverse its criteria.[11] Furthermore, states are required to readopt their water quality standards every

[11] In some cases states gradually adopt NNCs over several years as criteria are developed to cover

three years.

Four baseline specifications are estimated. Model (1a) includes water quality measures and non-point source contribution measures (population density and agricultural factors). Model (1b) adds preference-related variables (recreation consumption and political factors). Model (1c) adds state characteristics (gender, age, education, personal income, and industry shares). Model (1d) includes an indicator variable for EPA regions.

As a robustness check, I estimate each of the baseline specifications using alternative versions of the water quality measure. Baseline estimates are based on the overall count of impaired waters. Alternative versions break down those counts by waterbody type (lake, river, estuary), and use scaled versions of the cardinal measures, also broken down by waterbody type.

Permit limit regressions are run using N limits, P limits, and both N and P limits as outcomes. Models (2a), (2c), and (2e) include lagged NNC adoption and water quality measures as regressors. Models (2b), (2d), and (2f) additionally control for industry shares. Where the outcome variables is specified as a count of limits, I estimate versions of the models controlling for the overall total.

2.7 Econometric Results

Table 2.6 reports baseline probit results using contemporaneous factors. For reference, Table 2.9 reports analogous OLS results. The probit estimates can be interpreted for sign and significance of the regressors, or for predicted impacts of particular changes. Estimated impacts will vary based on initial values, making results difficult to generalize.

Impaired waters are significant in most of the Table 2.6 specifications. Estimates suggest non-nutrient related impacts are negatively related while nutrient related impacts are

additional waterbodies.

positively related. Note that is not possible to comment on the causal direction of this relationship. A state that adopts an NNC should potentially have more impairment listings in subsequent cycles. Once designated impaired, a particular waterbody will remain on the list during subsequent cycles until a TMDL is established. Thus even lagging the measure will not break the connection. Water area per person (size of water cover relative to the population) is negatively related to adoption on an NNC, while population density is positively related. Both are statistically significant in most specifications. Agricultural factor results are mixed. Live stock revenue from pigs has a negative and statistically significant relationship in all specifications. Farm spending on fertilizer has a positive but not statistically significant relationship in all specifications. Democrats in the state-level House of Representatives and federal House of Representative's LCV scores are both negatively related to NNC adoption, with statistically significant estimates in all specifications. Democrats in the state Senate on the other hand has a positive and statistically significant relationship with NNC adoption in all specifications. Recreational consumption spending and governor party affiliation results are mixed. State characteristics results are also reported in the table. One in particular that jumps out is that the portion of the population with a bachelors degree or higher has a positive and statistically significant relationship with NNC adoption.

Table 2.7 shows the same set of probit results using water impairment counts broken down by waterbody type. The same regressor sets are used although some rows are omitted in printed results. Non-nutrient impairment estimates are negative for each waterbody type, but only rivers are statistically significant across all specifications. Results are less consistent with respect to N/P-related impairments when broken down by waterbody type. Table 2.8 reports results using the scaled cardinal water quality measures. These results suggest a split between rivers and lakes, with both nutrient and non-nutrient impaired lakes having positive, statistically significant relationships in all specifications, while both nutrient and non-nutrient impaired rivers have negative, statistically significant relationships in all specifications. Across all tables, water cover relative to the population and livestock revenue from pigs have the most persistently significant coefficients.

As a robustness check, in Table 2.10 I report results model 1(b) for a number of alternative specifications each including state fixed effects. These results support similar conclusions, though have stronger significance. The specifications include a linear probability model, probit model, logistic model, and conditional logistic model. One potentially relevant fixed state factor that comes to mind are the up stream and down stream relationships between states, at least in terms of where borders the lie. There are likely a number of other time invariant factors between states.

Table 2.11 reports permit limit regressions without controlling for overall total new limits. Results suggest that lagged adoption of an NNC has a positive and statistically significant relationship to N limits and to N and P limits. The relationship to P limits is positive, but not statistically significant. Interestingly, estimated coefficients for the variable NRC, numeric response criteria. This variable is equal to one for states initially adopting an NNC for a response but not a causal nutrient parameter. Estimates suggest those states impose fewer nutrient related permit limits. Nutrient-related impairment has a positive relationship in all specifications, which is statistically significant in all but one. Non-nutrient related impairment has a negative relationship, which statistically significant in all but one specification. Table 2.12 reports analogous results but controls for total limits imposed in all specifications. Again, there is a positive and statistically significant relationship between adopting an NNC and imposing N limits or N and P limits. The relationship with imposing P limits is negative but not statistically significant. NRC has a negative and statistically significant relationship in all specifications. Again, nutrient related impairment has a positive relationship, and non-nutrient related impairment has negative relationship.

2.8 Conclusion

This study provides evidence concerning factors related to a state's decision to adopt an NNCs. In particular, it examines how the effect of water quality, agricultural, non-point source, and political factors can help in understanding why a state might hesitate to adopt an NNC. It also looks at the relationship between adopting an NNC and imposing nutrient related permit limits. This study does not attempt to evaluate which approach to nutrient regulation is the best, nor does it attempt to evaluate the overall effectiveness of any approach.

I find a robust positive relationship between nutrient impaired waters and adoption of an NNC but a robust negative relationship between non-nutrient impaired waters and adoption of an NNC. This may be explainable in terms of resource limitations and setting priorities. A caveat to this finding, however, is that measures of impairment do not factor in when a water is first designated impaired and are not consistently available for all states in all time periods. Furthermore, impairment designations depend on assessments which potentially vary for a number of reasons.

I also find a significant persistent negative relationship between squares miles of water per person. Larger water per person may suggest, among other things, fewer available resources to devote to developing NNCs for any particular set of waterbodies. It may also relate to nutrient impairment potential.

In terms of agricultural factors, results are mixed with one exception. There is a persistent negative relationship between pig revenue and adoption of an NNC across specifications. The relationship between NNC adoption and livestock presence is complicated by the fact that some livestock operations, CAFOs, are subject to regulation under the CWA, while others are not. A state with a large non-regulated livestock presence may hesitate to adopt an NNC as it would shift a disproportionate share of responsibility to point sources. The same reasoning applies to row crop presence. Trading programs, state specific regulation of non-point sources, and programs aimed at reducing fertilizer application may help alleviate such concerns. To account for the presence of non-agricultural non-point sources, I control for land area population density. Future work could do more to study the role of non-point sources. For example, new building permits or motor vehicle use could potentially be used as a control variables. In the case of new building permits, it may be possible to distinguish those in MS4 or other NPDES covered areas from those outside such areas.

In terms of the relationship between political affiliations and adopting an NNC, one of the interesting observations, is the split between house and senate democrat percents. State senate democrat affiliation has a persistent positive relationship with adoption of an NNC. At the same time state house democrat affiliation has a persistent negative relationship. This could be driven by correlation in the variables. Alternatively, it could reflect a difference between house and senate members. One possibility is that house representatives are more focused on particular issues within their districts, while senators tend to focus more on bigger picture concerns.

The second outcome explored was imposition of permit limits following adoption of an NNC. A potential concern in this context is endogeneity, although the use of lagged variables helps to mitigate this concern. Future research could address this point further. Waterbody specific regulations offer rich variation within and across states that could potentially be used in doing so. The permit limit results indicate that likely N/P dischargers in NNC states are more likely to be given N limits, but not necessarily P limits. Results also indicate that NRC state facilities are less likely to have nutrient permit limits. This supports EPAs position that response variable criteria are insufficient, at least for the purpose of permit limits. That said, future work should include other dimensions of implementation to get a more complete and meaningful picture.

2.9 Tables and Figures

Table 2.1: EPA Action and Guidance on Nutrient Policy

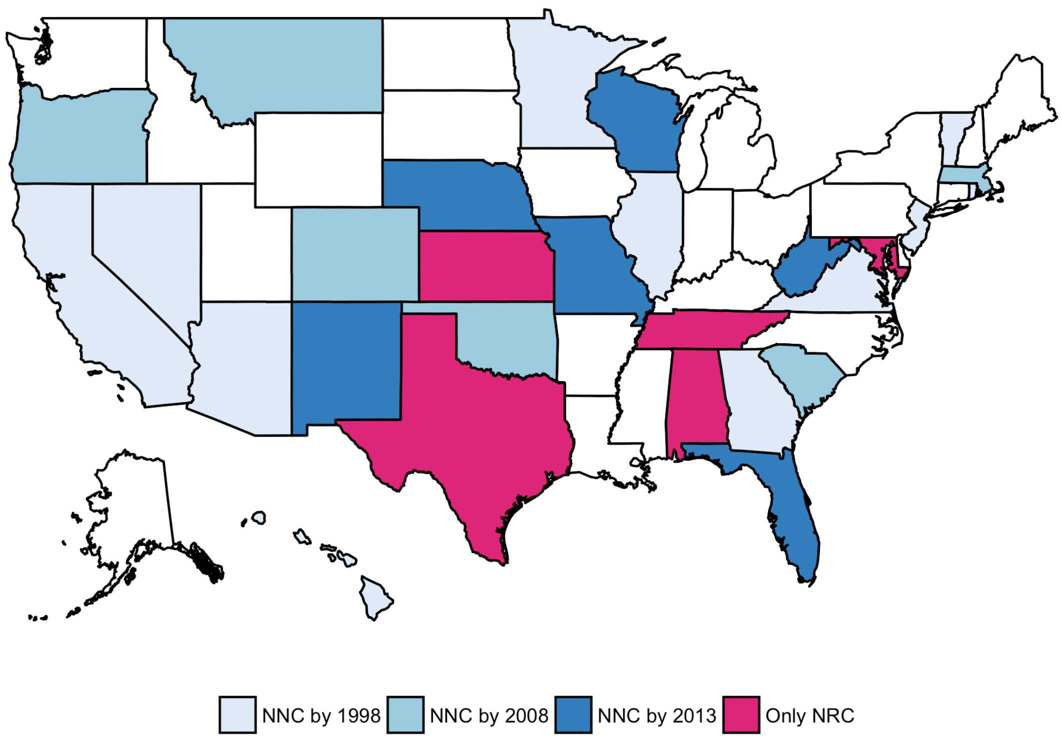
Date	Action
February 1998	Clean Water Action Plan calling for establishment of numeric nutrient criteria
June 1998	National Strategy
November 1999	Protocol for Developing Nutrient TMDLs
April 2000	Technical Guidance, Lakes and Reservoirs
July 2000	Technical Guidance, Rivers and Streams
January 2001	Notice of Nutrient Criteria in Federal Register
October 2001	Technical Guidance, Estuarine and Coastal Marine
November 2001	Grumbles Memo, instructions to states
May 2007	Grumbles Memo, renewed effort calling on states to "take bold steps"
June 2008	Technical Guidance, Wetlands
December 2008	Report on state adoption of numeric nutrient criteria
August 2009	Nutrient Innovations Task Group, "Urgent Call to Action"
August 2009	Evaluation Report, 'need to accelerate adoption'
March 2011	Stoner Memo, 'reaffirming the commitment'
September 2016	Beauvais Memo, 'renewed call to action'

Table 2.2: State Adoption of NNC

Year	Adopted an N/P NNC	Adopted a Chlor A NNC
1998	AZ, CA, GA, HI, IL, MN, NJ, NV, RI, VA, VT	
2000		AL
2001	SC	
2002	MT	MD
2004	OK, OR	
2005		TX
2006	MA	TN
2008	*CO*	
2009	MO	
2010	FL, WI	
2011	WV	
2013	*NE*, *NM*	
2014		*KS*

Earliest year state had least partially adopted an NNC. Italics indicates exact year unknown – states are known to have adopted policy by the year indicated. Analysis begins in 2004. States that adopted an NNC prior to that are listed but exact years are not recorded. *Sources*: EPA 2018a, state Nutrient Criteria Development Plans.

Figure 2.1: State Nutrient Approaches Map



Blue denotes states that had adopted an NNC by the indicated year. NRC states had only adopted an NRC by the end of the period examined. Policy classifications based on those used by EPA.

Table 2.3: N/P Permit Limits in Facility-Year Sample

	Operative Limits	Monitoring without Limits
Only N	6,553 (4.8%)	15,615 (11.4%)
Only P	10,123 (7.4%)	17,084 (12.5%)
Both N and P	2,858 (2.1%)	10,656 (7.8%)

Numbers are based on the sample of 136,814 facility-year observations from facilities likely to discharge N or P (described in the Section 5.2 of the text). Analysis covers limits that became effective from 2004 to 2014. Sources: EPA and author calculation.

Table 2.4: State Water Quality Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Max
Imputed Water Quality	550	0.205	0.404	0	1
Count Measures:					
Impaired Waters	550	1,237	1,774	60	13,346
Impaired Lakes	550	188	380	0	2,793
Impaired Rivers	550	970	1,527	25	11,384
Impaired Estuaries	550	86	220	0	1,703
N/P Impaired Waters	550	322	426	1	2,429
N/P Impaired Lakes	550	61	89	0	530
N/P Impaired Rivers	550	230	366	0	2,409
N/P Impaired Estuaries	550	34	130	0	1,159
Cardinal Measures:					
Impaired Lakes (acres)	550	252,592	510,075	0	3,745,678
Impaired Rivers (miles)	550	9,314	10,087	94	61,085
Impaired Estuaries (sq miles)	550	2,974	33,623	0	775,815
N/P Impaired Lakes (acres)	550	111,242	198,345	0	1,210,712
N/P Impaired Rivers (miles)	550	3,014	4,376	0	33,764
N/P Impaired Estuaries (sq miles)	550	1,043	16,803	0	393,274
Scaled Cardinal Measures:					
Impaired Lakes	550	113.8	138.6	0	512.5
Impaired Rivers	550	7.62	11.17	0.003	71.23
Impaired Estuaries	550	0.36	4.50	0	105.34
N/P Impaired Lakes	550	58.21	97.53	0	451.19
N/P Impaired Rivers	550	1.91	3.62	0	34.27
N/P Impaired Estuaries	550	0.172	2.28	0	53.4

Based on data reported for 2002 to 2016 cycles. Waterbodies are classified "N/P Impaired" if they are impaired and the cause of impairment is recorded as: nutrients, noxious aquatic plants, algal growth, organic enrichment/oxygen depletion, or turbidity. Ammonia impairments are not included. Scaled measures are weighted by square miles of water in the state. See text for additional details. Sources: EPA, Census, and author's calculation.

Table 2.5: Summary Statistics, 50 US States, 2004-2014

Statistic	N	Mean	St. Dev.	Min	Max
State Characteristics					
Male (%)	550	49.34	0.75	48.2	52.3
Young Age (%)	544	14.21	0.86	12.1	18.7
Prime Age (%)	550	40.8	1.6	36.8	45.2
Old Age (%)	550	19	2.2	12	26
Bachelor-Higher Educ (%)	550	28.9	5.4	15.8	43.4
Log Personal Inc Per Cap	550	10.6	0.18	10.1	11.1
Pop Density	550	194	258.6	1.16	1,214
Water/Pop	550	0.003	0.019	0.0001	0.14
Rec G/S PCEs (%)	550	6.9	0.731	4.3	8.7
Growth Rate	550	1.41	2.68	-9.33	17
Unemployment Rate	550	6.3	2.2	2.6	13.7
Industry Shares (%)					
Natural Resource/Mining	550	4.5	6.3	0.14	34.6
Construction	550	4.7	1.2	2.8	11.1
Manufacturing	550	12.4	5.7	1.5	32.2
Trade	550	17.4	2.4	10.7	23.2
Financial	550	18.6	5.2	9.8	43.2
Prof Business Services	550	10.2	2.8	3.6	19.4
Education/Health	550	8.3	1.8	3.8	13.3
Leisure/Hospitality	550	4.0	2.2	2.5	18.8
Public Administration	550	14	3.1	9.1	25.6
Political Factors					
Governor Democrat	550	0.467	0.499	0	1
Senate Democrat (%)	539	49.4	17.6	13.3	96
Senate Republican (%)	539	50.2	17.6	4	86.7
House Democrat (%)	539	50.5	16.1	13.3	92
House Republican (%)	539	49.2	16.2	8	86.7
House LCV	550	47.1	28.23	0	100
Agricultural Factors					
Fertilizer Expenditure	550	0.649	0.753	0.002	4.430
Feed Expenditure	550	0.923	1.099	0.003	6.220
Meat Revenue	550	1.949	2.908	0.005	16.266
Cattle Revenue	550	1.395	2.446	0.001	15.483
Pig Revenue	550	0.451	1.101	0.0002	10.080
Poultry Revenue	550	0.713	1.040	0.000	5.552
Other LS Revenue	550	0.103	0.120	0.002	1.140
Dairy Revenue	550	0.672	1.205	0.001	9.358

See text for description and source information. Missing political factor observations are from Nebraska, which has a unicameral, nonpartisan state legislature. Missing observations of young population are from Hawaii. Agricultural factors are all divided by one million.

Table 2.6: Probit NNC Regression Results

	(1a)	(1b)	(1c)	(1d)
Non-N/P Impaired (n)	-0.0004***	-0.0004***	-0.0004***	-0.0006***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)
N/P Impaired (n)	0.0008***	0.0009***	0.0002	-0.0004
	(0.0002)	(0.0002)	(0.0003)	(0.0004)
Imputed WQ	0.1973	-0.0061	-0.2248	-0.7901**
	(0.1637)	(0.1752)	(0.2286)	(0.3239)
Water/Population	-232.1575***	-396.2697***	-364.8467***	-338.7930**
	(72.0582)	(90.9970)	(115.8992)	(150.1445)
Population Density	0.0006**	0.0002	0.0018***	0.0025**
	(0.0002)	(0.0003)	(0.0007)	(0.0010)
Fertilizer Spending	0.0538	0.2462	0.3509	0.1543
	(0.1223)	(0.1524)	(0.2361)	(0.3660)
Feed Spending	0.2813**	0.2612	0.0327	-0.5941**
	(0.1281)	(0.1685)	(0.1997)	(0.2904)
Cattle Revenue	-0.0478	-0.0074	0.0759	0.3841***
	(0.0353)	(0.0465)	(0.0575)	(0.0923)
Pig Revenue	-0.3093***	-0.4423***	-0.3091**	-0.5209**
	(0.0820)	(0.0994)	(0.1216)	(0.2309)
Poultry Revenue	-0.0455	-0.0807	0.1986	0.8133***
	(0.0812)	(0.1016)	(0.1483)	(0.2373)
Recreational PCEs		0.2065**	-0.0408	0.5011**
		(0.0956)	(0.1706)	(0.2411)
Democrat Governor		-0.2234	0.2265	0.3878*
		(0.1360)	(0.1786)	(0.2326)
Senate Democrat (%)		0.0626***	0.1053***	0.1263***
		(0.0081)	(0.0135)	(0.0175)
House Democrat (%)		-0.0236***	-0.0870***	-0.0946***
		(0.0085)	(0.0157)	(0.0179)
House LCV Score		-0.0110***	-0.0148***	-0.0247***
		(0.0036)	(0.0054)	(0.0075)

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Probit NNC Regressions, *Table 2.6 continued*

	(1a)	(1b)	(1c)	(1d)
Male Population			0.7625**	-0.1293
			(0.3316)	(0.6060)
Old Age Population			0.1483**	0.1633*
			(0.0627)	(0.0880)
Bachelor Educ			0.0932**	0.2025***
			(0.0382)	(0.0554)
Log Pers Inc Per Cap			-0.4905	0.5843
			(1.1758)	(1.5261)
Natural Resource/Mining			-0.1279***	0.0724
			(0.0397)	(0.0589)
Construction			-0.0884	0.0056
			(0.1045)	(0.1626)
Trade			-0.0169	-0.0936
			(0.0615)	(0.0900)
Profess Business Serv			0.1750***	0.0894
			(0.0610)	(0.0767)
Financial			-0.1601***	-0.1750***
			(0.0350)	(0.0404)
Leisure/Hospitality			0.4597***	0.5820***
			(0.1174)	(0.2187)
Education/Health			0.0493	0.4466***
			(0.0771)	(0.1330)
Constant	-0.0260	-2.6057***	-37.8801*	-17.7135
	(0.1427)	(0.7328)	(19.9025)	(34.2558)
Region Dummies				✓
Observations	550	539	539	539
Log Likelihood	-328.3528	-272.3178	-183.4569	-140.9481
Akaike Inf. Crit.	678.7056	576.6356	420.9139	353.8962
Note:		*p<0.1; **p<0.05; ***p<0.01		

Table 2.7: Probit NNC Regression Results, Waterbody Type Count WQ Measures

	(1a)	(1b)	(1c)	(1d)
Non-N/P Impaired Lakes (n)	0.0001	0.0002	-0.0005	-0.0008
	(0.0003)	(0.0003)	(0.0004)	(0.0007)
Non-N/P Impaired Rivers (n)	-0.0004***	-0.0004***	-0.0003**	-0.0004*
	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Non-N/P Impaired Estuaries (n)	-0.0015	-0.0017	-0.0019**	-0.0018
	(0.0010)	(0.0015)	(0.0010)	(0.0012)
N/P Impaired Lakes (n)	0.0021**	0.0014*	0.0015	0.0004
	(0.0008)	(0.0008)	(0.0010)	(0.0013)
N/P Impaired Rivers (n)	0.0005	0.0006*	0.000001	-0.0006
	(0.0003)	(0.0003)	(0.0004)	(0.0005)
N/P Impaired Estuaries (n)	0.0037	0.0058	-0.0017	-0.0002
	(0.0023)	(0.0037)	(0.0014)	(0.0019)
Imputed WQ	0.2517	-0.0109	-0.1748	-0.7596**
	(0.1721)	(0.1856)	(0.2320)	(0.3262)
Water/Population	-260.6110***	-431.2657***	-340.6144***	-293.4894*
	(75.6764)	(97.2897)	(117.9974)	(154.5448)
Population Density	0.0004	-0.0001	0.0022***	0.0028***
	(0.0003)	(0.0003)	(0.0007)	(0.0010)
Fertilizer Spending	0.0124	0.2177	0.2978	0.1395
	(0.1282)	(0.1557)	(0.2478)	(0.3760)
Feed Spending	0.2823**	0.2293	0.0762	-0.5347*
	(0.1301)	(0.1669)	(0.2086)	(0.2966)
Cattle Revenue	-0.0432	0.0067	0.0704	0.3753***
	(0.0353)	(0.0460)	(0.0587)	(0.0923)
Pig Revenue	-0.3595***	-0.4569***	-0.3273**	-0.5048**
	(0.0906)	(0.1032)	(0.1309)	(0.2416)
Poultry Revenue	-0.0264	-0.0587	0.2014	0.8034***
	(0.0823)	(0.1013)	(0.1536)	(0.2394)
Recreational PCEs		0.2190**	-0.0114	0.5237**
		(0.0980)	(0.1728)	(0.2437)
Constant	-0.0320	-2.7262***	-39.0557*	-21.3184
	(0.1468)	(0.7607)	(20.5457)	(37.0447)
Political Factors		✓	✓	✓
State Characteristics			✓	✓
Industry Shares			✓	✓
EPA Region Dummies				✓
Observations	550	539	539	539
Log Likelihood	-318.5436	-265.8147	-180.6032	-140.3220
Akaike Inf. Crit.	667.0873	571.6293	423.2064	360.6441
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 2.8: Probit NNC Results, Scaled Cardinal WQ Measures

	(1a)	(1b)	(1c)	(1d)
Non-N/P Impaired Lakes	0.0027***	0.0027***	0.0020*	0.0073***
(acres/sq. miles)	(0.0007)	(0.0008)	(0.0011)	(0.0021)
Non-N/P Impaired Rivers	-0.0259***	-0.0365***	-0.0521***	-0.0935***
(miles/sq. miles)	(0.0086)	(0.0086)	(0.0117)	(0.0210)
Non-N/P Impaired Estuaries	-1.0788**	-0.9769*	-1.2204	-1.2353
(sq. miles/sq. miles)	(0.4486)	(0.5876)	(0.7465)	(1.2554)
N/P Impaired Lakes	0.0029***	0.0030***	0.0040***	0.0075***
(acres/sq. miles)	(0.0008)	(0.0009)	(0.0015)	(0.0020)
N/P Impaired Rivers	-0.0525**	-0.0522*	-0.1187***	-0.2149***
(miles/sq. miles)	(0.0237)	(0.0276)	(0.0372)	(0.0465)
N/P Impaired Estuaries	1.0562***	1.1070***	1.3609***	1.8339**
(sq. miles/sq. miles)	(0.3444)	(0.3837)	(0.4939)	(0.7649)
Imputed WQ	0.3251*	0.1912	-0.3618	-0.8089**
	(0.1662)	(0.1869)	(0.2437)	(0.3709)
Water/Population	-224.6808***	-506.5876***	-454.7491***	-806.8795*
	(86.7067)	(107.9854)	(171.9124)	(437.9112)
Population Density	0.0009***	0.0005	0.0027***	0.0028**
	(0.0003)	(0.0004)	(0.0007)	(0.0012)
Fertilizer Spending	0.1772	0.3592**	0.6568***	0.0215
	(0.1197)	(0.1449)	(0.2453)	(0.4954)
Feed Spending	0.1702	0.1810	-0.2322	-0.5929*
	(0.1313)	(0.1827)	(0.2144)	(0.3400)
Cattle Revenue	0.0072	0.0520	0.2364***	0.6294***
	(0.0380)	(0.0549)	(0.0693)	(0.1262)
Pig Revenue	-0.3950***	-0.5081***	-0.3448***	-0.6146**
	(0.0901)	(0.1069)	(0.1272)	(0.2723)
Poultry Revenue	0.0219	-0.1151	0.1225	0.5232*
	(0.0856)	(0.1084)	(0.1568)	(0.2675)
Recreational PCEs		0.1334	-0.2542	-0.1594
		(0.1000)	(0.1881)	(0.3014)
Constant	-0.3292*	-2.1784***	-27.5957	68.8977
	(0.1785)	(0.8011)	(21.3064)	(42.5180)
Political Factors		✓	✓	✓
State Characteristics			✓	✓
Industry Shares			✓	✓
EPA Region Dummies				✓
Observations	550	539	539	539
Log Likelihood	-319.8129	-259.4783	-170.3744	-116.8246
Akaike Inf. Crit.	669.6257	558.9567	402.7489	313.6493

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.9: OLS NNC Regression Results

	(1a)	(1b)	(1c)	(1d)
Non-N/P Impaired (n)	-0.0001***	-0.0001***	-0.0001***	-0.0001***
	(0.00001)	(0.00001)	(0.00001)	(0.00001)
N/P Impaired (n)	0.0002***	0.0002***	0.0001*	-0.00001
	(0.0001)	(0.0001)	(0.00004)	(0.00004)
Imputed WQ	0.0737	0.0029	-0.0077	-0.0704
	(0.0580)	(0.0538)	(0.0498)	(0.0463)
Water/Population	-2.9002***	-3.9584***	-2.4900***	-5.5957***
	(0.3161)	(0.4579)	(0.9571)	(1.4622)
Population Density	0.0003***	0.0002**	0.0005***	0.0003**
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Fertilizer Spending	0.0133	0.0524	0.0818**	0.0481
	(0.0405)	(0.0346)	(0.0349)	(0.0449)
Feed Spending	0.0748**	0.0399	0.0123	-0.1088**
	(0.0334)	(0.0325)	(0.0352)	(0.0458)
Cattle Revenue	-0.0033	0.0176	0.0207	0.0777***
	(0.0109)	(0.0109)	(0.0126)	(0.0159)
Pig Revenue	-0.0857***	-0.1057***	-0.0655***	-0.0075
	(0.0140)	(0.0136)	(0.0150)	(0.0247)
Poultry Revenue	0.0112	0.0074	0.0529	0.1019***
	(0.0267)	(0.0324)	(0.0336)	(0.0336)
Recreational PCEs		0.0495*	-0.0294	0.0385
		(0.0285)	(0.0334)	(0.0327)
Democrat Governor		-0.0556	0.0365	0.0511
		(0.0426)	(0.0415)	(0.0356)
Senate Democrat (%)		0.0197***	0.0201***	0.0225***
		(0.0022)	(0.0019)	(0.0018)
House Democrat (%)		-0.0077***	-0.0146***	-0.0178***
		(0.0028)	(0.0023)	(0.0022)
House LCV Score		-0.0039***	-0.0034***	-0.0043***
		(0.0010)	(0.0011)	(0.0010)

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OLS NNC Regressions, *Table 2.9 continued*

	(1a)	(1b)	(1c)	(1d)
Male Population			0.1225*	-0.1007
			(0.0668)	(0.0789)
Old Age Population			0.0379***	0.0173
			(0.0118)	(0.0120)
Bachelor Educ			0.0307***	0.0418***
			(0.0074)	(0.0076)
Log Pers Inc Per Cap			-0.4833**	-0.2136
			(0.2297)	(0.2214)
Natural Resource/Mining			-0.0123*	0.0156**
			(0.0065)	(0.0063)
Construction			-0.0009	-0.0042
			(0.0229)	(0.0205)
Trade			0.0066	-0.0045
			(0.0118)	(0.0136)
Profess Business Serv			0.0384***	0.0264**
			(0.0115)	(0.0118)
Financial			-0.0239***	-0.0254***
			(0.0043)	(0.0049)
Leisure/Hospitality			0.0691***	0.0574***
			(0.0105)	(0.0100)
Education/Health			-0.0018	0.0688***
			(0.0150)	(0.0195)
Constant	0.3737***	-0.3261	-2.3890	4.9323
	(0.0416)	(0.2193)	(3.6900)	(4.1641)
Region Dummies				✓
Observations	550	539	539	539
R^2	0.1343	0.2772	0.4933	0.5824
Adjusted R^2	0.1182	0.2565	0.4676	0.5533
Residual Std. Error	0.4686	0.4306	0.3644	0.3338
F Statistic	8.3615***	13.3716***	19.1715***	20.0419***
Note:		*p<0.1; **p<0.05; ***p<0.01		

Table 2.10: Robustness Check, NNC Regression Alternative Specifications

	LPM	*Probit*	*Logistic*	*Conditional Logistic*
	(1b)	(1b)	(1b)	(1b)
Non-N/P Impaired	-0.00001	0.0023***	0.0075***	-0.0001***
	(0.00001)	(0.0001)	(0.0008)	(0.0000)
N/P Impaired	0.00004	0.1680***	0.5602***	0.0018***
	(0.00003)	(0.0029)	(0.0173)	(0.0000)
Water/Population	3.4194	686.3201***	2,217.5670***	-16,923.6500***
	(2.4886)	(42.5390)	(287.5145)	(0.0000)
Population Density	0.0060***	3.8480***	13.0287***	0.0072***
	(0.0022)	(0.0483)	(0.4050)	(0.0000)
Fertilizer Spending	0.0143	21.8594***	69.5213***	0.0196***
	(0.0387)	(0.7841)	(7.9553)	(0.0000)
Feed Spending	0.1016**	34.3224***	116.0907***	0.4980***
	(0.0467)	(0.6702)	(6.5930)	(0.0000)
Cattle Revenue	-0.0034	-16.8942***	-55.5174***	-0.2642***
	(0.0400)	(0.3936)	(2.2102)	(0.0000)
Pig Revenue	-0.0447	-0.9396*	-4.3005	-0.4578***
	(0.0329)	(0.5049)	(5.0730)	(0.0000)
Poultry Revenue	-0.1363***	-50.0201***	-166.3208***	-0.5159***
	(0.0402)	(1.2005)	(8.3933)	(0.0000)
Recreational PCEs	-0.1333***	-27.7720***	-91.1585***	-1.0321***
	(0.0448)	(0.9279)	(6.0396)	(0.0000)
Democrat Governor	0.0315	-7.0577***	-22.1101***	0.5565***
	(0.0294)	(0.4015)	(2.5221)	(0.0000)
Senate Democrat	0.0045**	-0.5260***	-1.6960***	0.0162***
	(0.0021)	(0.0242)	(0.1580)	(0.0000)
House Democrat	-0.0048**	0.5493***	1.7306***	-0.0311***
	(0.0021)	(0.0557)	(0.3495)	(0.0000)
House LCV Score	-0.0021***	-0.6729***	-2.2242***	-0.0074***
	(0.0008)	(0.0147)	(0.0849)	(0.0000)
Constant	0.4000	68.3015***	226.8753***	
	(0.3189)	(2.8392)	(18.3330)	
State FEs	✓	✓	✓	✓*
Observations	539	539	539	539
R^2	0.8568			0.0723
Adjusted R^2	0.8382			
Max. Possible R^2				0.1774
F Statistic	45.9446***			
LR Test				40.4633***
Score (Logrank) Test				62.7558***
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 2.11: OLS N/P Permit Limit Regression Results

	New N Limit		New P Limit		New N and P Limits	
	(2a)	(2b)	(2c)	(2d)	(2e)	(2f)
Lag NNC	9.5252**	11.9825***	5.9378	5.8829	4.9684**	6.5978**
	(3.9481)	(3.8245)	(3.7804)	(4.5166)	(2.5338)	(2.6961)
Lag NRC	-3.4496***	-2.1244	-3.4859	-5.5396	-0.9576**	-0.5084
	(0.8984)	(1.5291)	(3.0472)	(3.8399)	(0.4348)	(0.8519)
Non-N/P Imp	-0.0019***	-0.0014***	-0.0006	-0.0011	-0.0009**	-0.0007**
	(0.0005)	(0.0005)	(0.0006)	(0.0007)	(0.0004)	(0.0003)
N/P Imp	0.0074***	0.0047**	0.0063***	0.0024	0.0067***	0.0051***
	(0.0018)	(0.0019)	(0.0022)	(0.0023)	(0.0014)	(0.0014)
Nat Rsrc/Min		0.9584***		-0.4930		0.5201***
		(0.2347)		(0.5390)		(0.1416)
Construction		0.0004		-1.9075*		0.1563
		(0.8386)		(1.1555)		(0.4842)
Trade		1.3342***		1.0758**		0.8402**
		(0.4850)		(0.5306)		(0.3749)
Prof/Bus Serv		2.1633***		1.4123*		1.2585***
		(0.5909)		(0.7517)		(0.4456)
Fin		1.1613**		-0.0318		0.5790**
		(0.4648)		(0.4661)		(0.2769)
Leis/Hosp		0.4637		-0.9416		0.3171**
		(0.2966)		(0.5771)		(0.1592)
Edu/Health		1.3710***		-0.5104		0.9097***
		(0.5150)		(0.9233)		(0.2626)
Constant	5.3361***	-79.3845***	10.2497***	-1.4447	0.9064	-49.3810***
	(1.0494)	(19.6452)	(3.0261)	(35.7987)	(0.5976)	(13.7177)
Observations	547	547	547	547	547	547
R^2	0.0303	0.0759	0.0105	0.0367	0.0346	0.0685
Adjusted R^2	0.0214	0.0551	0.0014	0.0150	0.0257	0.0476
Res. Std. Err.	30.6331	30.0999	38.8859	38.6195	19.0307	18.8157
F Statistic	3.3831***	3.6548***	1.1509	1.6935*	3.8790***	3.2733***

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.12: OLS N/P Permit Limit Regression Results 2

	New N Limit		New P Limit		New N and P Limits	
	(2a)	(2b)	(2c)	(2d)	(2e)	(2f)
Lag NNC	5.3982**	6.8330***	-1.1354	-2.2705	2.5025*	3.5404**
	(2.1588)	(2.1964)	(3.5947)	(4.6955)	(1.3018)	(1.4882)
Lag NRC	-11.5953**	-9.3992**	-17.4468**	-17.0581**	-5.8246*	-4.8276*
	(4.5983)	(4.1274)	(6.9761)	(6.8997)	(2.9951)	(2.6700)
New Lims	0.0515**	0.0577***	0.0883***	0.0914***	0.0308**	0.0343**
	(0.0203)	(0.0211)	(0.0235)	(0.0244)	(0.0140)	(0.0146)
Non-N/P Imp	-0.0018***	-0.0009**	-0.0004	-0.0003	-0.0008**	-0.0004
	(0.0005)	(0.0004)	(0.0005)	(0.0005)	(0.0003)	(0.0003)
N/P Imp	0.0091***	0.0088***	0.0092***	0.0088***	0.0077***	0.0075***
	(0.0018)	(0.0021)	(0.0019)	(0.0025)	(0.0014)	(0.0015)
Nat Rsrc/Min		1.2438***		-0.0411		0.6895***
		(0.2846)		(0.3418)		(0.1898)
Const		1.7123**		0.8030		1.1727***
		(0.6964)		(1.1304)		(0.4057)
Trade		0.6079		-0.0741		0.4090
		(0.4120)		(0.5809)		(0.2787)
Prof Bus Serv		1.1648***		-0.1687		0.6656**
		(0.4451)		(0.9002)		(0.3050)
Fin		1.8068***		0.9902***		0.9622***
		(0.5811)		(0.3227)		(0.3705)
Leis/Hosp		0.8826***		-0.2782		0.5659***
		(0.2901)		(0.3475)		(0.1760)
Edu/Hlth		2.3661***		1.0652**		1.5005***
		(0.5951)		(0.5404)		(0.3634)
Constant	-2.7647	-97.5915***	-3.6342	-30.2727	-3.9337**	-60.1909***
	(2.7774)	(22.0375)	(2.2170)	(24.6513)	(1.8909)	(15.7366)
Observations	547	547	547	547	547	547
R^2	0.2340	0.3107	0.3895	0.4094	0.2222	0.2820
Adjusted R^2	0.2255	0.2938	0.3827	0.3949	0.2136	0.2645
Res. Std. Err.	27.2508	26.0213	30.5731	30.2684	17.0975	16.5351
F Statistic	27.5004***	18.4768***	57.4164***	28.4151***	25.7144***	16.1016***

Note: *p<0.1; **p<0.05; ***p<0.01

Chapter 3

The Impact of Unemployment Insurance Receipt on Nonemployment Duration and Subsequent Job Quality: Evidence from the U.S.

3.1 Introduction

The US unemployment insurance (UI) system provides weekly payments to qualifying workers who have lost their jobs. In 2016, 32 billion dollars in unemployment insurance benefits were paid to 6.2 million beneficiaries in the US (US Dep't of Labor). Roles served by the UI system from an economic theory perspective include facilitating efficient search, smoothing consumption, and potentially serving as an automatic stabilizer during recessions. If benefits result in job quality improvement, there are also potential productivity gains. From individual quality of life and social perspectives, UI benefits potentially play a hugely important role in protecting cash-strapped individuals from the whims of industry shocks and reallocations. The major concern surrounding UI benefits, is that by paying individuals while they remain out of work, the system creates incentives for individuals to remain out of work longer, resulting in resource underutilization. This is a moral hazard concern. Benefit availability could also increase individual's willingness to accept more risky employment, cause individuals to put less effort into retaining employment, and crowd out forms of self-insurance (such as private savings, and spousal employment).

Theoretically, UI benefits are expected to increase unemployment durations, although not necessarily through a moral hazard effect. The impact on job quality is theoretically ambiguous. Empirical evidence supports the view of UI benefits as increasing unemployment durations, in terms of both benefit level (Landais 2015), and benefit duration (Card, Chetty, and Weber 2007b). Results on job quality are mixed, though tend to indicate negative or no impacts (Schmieder, von Wachter, and Bender 2016; Le Barbanchon 2016; Caliendo, Kunn, and Uhlendorff 2016; van Ours and Vodopivec 2008; Lalive 2007). Nekoei and Weber (2017) recently argue that the impact is positive after taking into account negative duration dependence. Some US studies have found positive impacts, although none have recently looked the question.

This paper looks at nonemployment duration and subsequent job quality among UI benefit recipients and non-recipients using four panels of U.S. survey data together covering most of 1995 to 2013. The term 'nonemployment' is used broadly herein to refer to the time until an individual finds employment following separation. Section 2 reviews related literature. Section 3 discusses the institutional background. Section 4 describes the data. Section 5 describes the empirical approach. Section 6 presents the results and Section 7 concludes.

3.2 Literature Review

This paper contributes to two areas of research. First, is that on how UI program features impact related durations. Second, is that on how UI program features impact subsequent job quality.

Early empirical work found evidence of large spikes in unemployment exit rates at around the time of and right before UI benefit exhaustion, suggesting that individuals were waiting to return to work until their UI benefits ran out (Meyer 1990; Katz and Meyer 1988). For individuals being recalled to a prior job, the impact was greater (Katz and Meyer 1988; 1990). More recent work indicates some of those effects were a consequence of how spells were being measured, but that benefits still do have an impact on duration even when measured differently (Card, Chetty, and Weber 2007b). Some of the increase in duration likely reflects greater search intensity and fewer transitions out of the labor market, rather than a pure moral hazard effect (Chetty 2008; Farber, Rothstein, and Valletta 2015).

Recent empirical studies on UI program parameters and subsequent job outcomes have mostly find negative or no impact on quality, most commonly measured by either earnings or job tenure.[1] These estimates have primarily been for European countries. Using Austrian data, Lalive (2007) and Card, Chetty, and Weber (2007a) find that severance pay and extended benefit duration eligibility increase nonemployment durations, but have no effect on subsequent job match quality (RDD). On the other hand, and also using Austrian data, Nekoei and Weber (2017) find positive match quality effects. They argue that select other studies are consistent with their findings once negative duration dependence is taken into account and that in fact positive impacts associated with UI benefits may offset some of the impact from negative duration dependence. The role of negative duration dependence is an interesting and important point.

The latest to look at job quality using U.S. data appear to be Centeno and Novo (2006), Centeno (2004), and McCall and Chi (2008). All three use NLSY data collectively covering 1979-2002 and find that more generous UI benefits have positive impacts on subsequent job quality.[2] Consistent with those results, in Canada, Belzil (2001) finds that maximum benefit duration has a positive impact on subsequent job duration.

Using German data, Caliendo, Tatsiramos, and Uhlendorff (2013), and Schmieder, von Wachter, and Bender (2014; 2016) find that potential benefit durations have no or negative job quality impacts (RDD). Using French data and focusing on "low employability workers," Le Barbanchon (2016) finds similar results. Using Slovenian data, van Ours and Vodopivec (2008) find that reducing potential benefit duration had no impact on subsequent wages or

[1] Others considered include region, occupation, and industry changes, firm-specific attributes, and probabilities of full versus part-time, and of permanent versus temporary.

[2] Centeno (2004) and Centeno and Novo (2006), using the NLSY male subsample covering 1979-1998, find positive impacts of UI generosity on starting wage and job tenure (proportional hazards model; quantile regression model). McCall and Chi (2008), use NLSY data extending to 2002, and find that weekly benefit amounts have a significant positive impact on re-employment wages, but that the impact dissipates with the length of the unemployment spell, reaching zero after about 34 weeks (hazards model).

job duration, or on the probability of finding a permanent as opposed to temporary job. Using Portuguese data, Centeno and Novo (2009) is sometimes portrayed as finding positive quality impacts, however, they find more evidence of negative than positive impacts.[3]

3.3 Institutional Background

In the U.S., many features of the UI system are set at the state level. Different states have different rules created within federal program parameters. To start a period of benefit receipt, a worker must first establish eligibility. This is done by looking at earnings during a base period. The standard base period used is the first four of the last five completed calendar quarters preceding the date of application for benefits. Many states allow alternative and modified base periods for individuals unable to establish eligibility based on the regular base period. The idea behind these eligibility requirements is to ensure sufficient attachment to the labor force among benefit recipients. Particular standards vary, but can be grouped into a number of types, all requiring sufficient earnings during the base period. Another typical requirement is that separation from employment have been involuntary, although there are exceptions. In some states, even a worker who has not fully separated from employment can establish benefit eligibility through short time compensation programs.

Once eligibility is established, base period earnings are again used to determine an individuals benefit amount and maximum potential benefit duration. Particulars vary here as well.

[3] They consider heterogeneity along two dimensions, essentially splitting estimates into eight separate boxes. Two of those boxes were associated with positive impacts. Combined average effects would likely be negative.

3.4 Data

This study uses data from the 1996, 2001, 2004, and 2008 Survey of Income and Program Participation (SIPP) panels. The panels range from 36 to 64 months long, and start with samples ranging from approximately 90,000 to approximately 105,000 individuals. Interviews are conducted every four months, although most variables are recorded on a monthly basis. Employment status is reported weekly. The data includes a rich set of information on surveyed individuals. Other papers using SIPP data in the unemployment context include Chetty (2008), Kroft and Notowidigdo (2016), and Rothstein and Valletta (2017), Fujita and Moscarini (2017).

3.4.1 Sample and Spell Construction

Following recommendations from the SIPP User's Guide, I only use observations which are assigned strictly positive longitudinal weights and do not use observations with either of the Type Z imputations. Fujita and Moscarini (2017) use the same restriction. This restricts the sample to individuals who participate in the entire panel and eliminates observations for which all labor force characteristics are imputed. The same longitudinal weights are used throughout much of the analysis.

I transform the individual-month level SIPP data into individual-spell level data based on weekly employment status. Following common practice in the literature, individuals are classified as either having a job (employed) or not having a job (nonemployed).[4] Status definition details are provided below. Initial spells begin with each individual's first observation in the panel (and are all left censored). Subsequent spells begin each time an individual is observed changing status (from employment to nonemployement, or from nonemployment to employment). In particular, new spells begin in the week that a new status is first observed.

[4] See e.g., Nekoei and Weber (2017); Barbanchon (2016); Schneider, von Watcher, and Bender (2016); Lalive (2007); Chetty, Card, and Weber (2007a).

The spell-based data is restricted to nonemployment spells for which data on pre and post spell employment are available. This is necessary for comparing job attributes before and after the spell. It also eliminates left censored nonemployment spells. Further, nonemployment spells are restricted to those with at least three months of preceding employment (Kroft and Notowidigdo 2016 and others impose this or similar restrictions). NU spells lasting longer than two years are dropped (following Nekoei and Weber (2017)). I also eliminate NU spells that do not last at least two weeks. Two weeks corresponds to the waiting and filing time period needed to apply for UI benefits (Addison and Blackburn 2000). I also drop individuals who transition between employed and nonemployed more than once in a single month.

Further, I drop spells for which the individual is less than 18 or greater than 70 years old as of the first month of the spell. Similar or more restrictive age restrictions are commonly imposed in related literature.[5] I also drop spells during which an individual reports being retired, or for which average weekly household income during the spell is less than 1 or greater than 10,000. Additional restrictions, discussed further below, will be based on the reported reason for job separation.

3.4.2 Variable Construction

Labor Force Status. Nonemployment duration is measured as the number of weeks that a nonemployment spell lasts. Employed status includes: (1) people who are working, and (2) people who are absent without pay but not on layoff. Nonemployed status includes: unemployed and not in the labor force. Unemployed status includes: (1) people on layoff absent without pay, and (2) people with no job who are looking or on layoff. Not in the

[5] For example, Nekoei and Weber (2017) look at individuals age 30 to 50 (though use an age cutoff regression discontinuity design), Centeno and Novo (2006) look at individuals 16 or older (and not enrolled in school), Farber, Rothstein, and Valletta (2015) look at individuals age 18 to 69.

labor force includes: (1) people with no job and not looking. These categories are mutually exclusive.

Earnings. Individual earnings are reported monthly for up to two concurrent jobs. For months in which more than one is reported, I use the sum of both. During months in which an individual transitions from employment to nonemployment or vice versa, all earnings are attributed to the portion of the month associated with employment. Number of weeks employed are used for the purpose of computing weekly averages. All dollar amounts are reported in November 2013 dollars.[6] The difference in earnings before and after a nonemployment spell is used as a measure of job quality. The more earnings increase (or the less they decrease), the more likely the job is of higher quality. All else constant, individuals should prefer higher wages. To measure the change in earnings, I use the difference in log average weekly earnings.[7] Note, I use monthly earnings as reported rather than using a reporting month earnings, such as in Ham and Shore-Sheppard (2005), because of the panel and spell construction structures.[8]

UI Program Participation. I distinguish between individuals who report receiving UI benefits and those who do not (non-recipients). This self-reported information is subject to measurement error and imprecision. In some cases, reported benefit receipt does not align well with reported employment status.

Resources and Constraints. Measures of total household income are looked at using the household in which the individual was a member during the first month of a given nonemployment spell as the reference household. Household income is all types of income received by all members of a household. The ratio of an individual's earnings to the household income, measures the individual's level of contribution from employment. The higher the

[6] Adjusted using a monthly PCE price index. FRED, PCEPI. Nominal values are used in assessing UI eligibility and where comparison is drawn to nominal values from other sources.

[7] Similar measures are used in Lalive (2007), Nekoei and Weber (2017), as well as others.

[8] In particular, I use weekly earnings. Weeks per month vary, thus reference month earnings may not translate to weekly for all months.

contribution, the more impact might be expected from job loss.

Individual and Household Characteristics. Unless otherwise noted, demographic and household characteristics are as of the first month of a spell. They include: age, gender, race, highest grade completed, school enrollment status, marital status, children under 18, household income, and home ownership status. I construct indicators for changes in highest level of education, marital status, children under 18, and home ownership status occurring during spells.

State Policy Variables. State UI programs vary along multiple important dimensions. Variables used here are the average weekly benefit and average maximum potential duration. State benefit data on average maximum potential duration is obtained from Department of Labor Table AR218. Observations with average maximum potential duration less than 15 weeks are dropped.[9] Durations afforded under extended benefit programs are not included.[10] Potential durations offer largely exogenous variation. The average maximum potential duration is based upon realizations, but still offers some exogenous variation. It is used as a control variable for job quality (wage) regressions, and to split the sample into high benefit and low benefit states in looking at nonemployment duration. Average weekly benefit duration is from the Department of Labor Monthly Program and Financial Data.

State-Level Business Cycle / Economic Conditions. Following others, I use the monthly unemployment rate during the first month of a spell to control for business cycle conditions (see e.g., Centeno and Novo 2006a). State-month unemployment rate data is from the BLS.

3.5 Empirical Framework

The two outcomes are nonemployment duration and job quality. Nonemployment duration is analyzed graphically using Kaplan-Meier survival curves to compare recipients and

[9] Illinois 2003-2004, Maryland 1998, Maine 2011, Utah 2003, Tennessee 2009.
[10] Future work could do this.

non-recipients. It is important to note that assignment into those statuses is not random. As a means of generating separation, I also analyze high and low benefit states separately. State-year observations in the sample are split into high and low benefit state groups based on the average maximum potential durations (PDs) recorded for each state-year. PDs above median are used to define the high benefit state group. PDs at or below median are used to define the low benefit state group. A similar approach is used in Chetty (2008). Spells are assigned to groups based on their initial state and year. In each group, I compare recipients and non-recipients. Non-recipients are not expected to respond to PD and should have similar survival curves in both high and low PD states. If recipients are responding to benefit levels, then their response is expected to be greater in high benefit states. I estimate a Cox Proportional Hazard model to assess the significance of observed survival curve differences.[11]

I then estimate change in job quality using an OLS model. The approach is similar to that in Addison and Blackburn (2001). I control for state policy related variables, which vary over time and across states. I also include specifications that control for earnings, which is believed to be strongly correlated with significant job attributes such as occupation.

3.6 Empirical Results

Figure 3.1 presents Kaplan-Meier survival curves for the full sample of nonemployment spells (including those that are right censored). The survival curves show the time until reemployment for each group. The graph shows a clear gap between recipients and non-recipients. Figure 3.2 shows the same graph split into high and low potential duration states. Both groups exhibit a similar notable gap between recipients and non-recipients.

[11] The hazard model is specified as follows: (risk of finding a job at time t)

$$h_i(t) \;=\; h_0(t)\exp(\beta_k X_{ik}),$$

where the baseline hazard function, $\alpha(t) = \log(h_0(t))$, is unspecified, X_{ik} are covariates, and t is the survival time (time remaining in nonemployed status).

Figure 3.3 shows the same graphs in Figure 3.2 but plots recipients with recipients and non-recipients with non-recipients. In panel (b) there is as expected almost no difference between non-recipients. In panel (a) there is a slight difference but in the opposite direction as expected. This is surprising and suggests that individuals are remaining out of work longer in lower potential duration states. Estimating a hazard model for the sample of recipients with an indicator for high potential duration states suggests the difference is significant at a five percent level, although with a positive coefficient (coef = 0.04916, $p = .0372$).[12]

Results for the wage equation regressions can be found in Tables 3.2 and 3.3. Table 3.3 is the same as Table 3.2 but includes spell duration as an independent variable. Results are mixed. While the for receiving UI benefits is mostly negative, it is mostly not significant. This suggests that there is not a significant difference in subsequent job quality, as measured by wage difference, between recipients and non-recipients.

3.7 Conclusion

This chapter does not find strong evidence of a negative relationship between receiving unemployment insurance benefits and subsequent job quality once relevant characteristics are controlled for. It finds suggestive evidence indicating that it may not be differences in potential duration that are driving observed differences in nonemployment duration, at least not entirely.

[12] Using the longitudinal weights the coefficient changes to 0.059751 and becomes more significant.

3.8 Tables and Figures

Table 3.1: All Panel NU Spell Sample Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Max
Duration (weeks)	17,462	18.61	19.27	2	104
Recieve UI	17,462	0.32	0.47	0	1
Find Job	17,462	1	0	1	1
Age	17,462	37.87	11.99	19	69
Sex					
Female	17,462	0.52	0.5	0	1
Male	17,462	0.48	0.5	0	1
Race					
White	17,462	0.83	0.37	0	1
Black	17,462	0.11	0.32	0	1
Asian	17,462	0.02	0.14	0	1
Other	17,462	0.04	0.18	0	1
Latino	17,462	0.07	0.26	0	1
Marital					
Married	17,462	0.50	0.50	0	1
Not Married	17,462	0.30	0.46	0	1
Marg Attach	17,462	0.18	0.38	0	1
Top Educ					
Less HS	17,462	0.14	0.35	0	1
HS	17,462	0.30	0.46	0	1
Some Coll	17,462	0.36	0.48	0	1
Bach Plus	17,462	0.20	0.40	0	1
Enroll	17,462	0.10	0.30	0	1
Living					
Own	17,462	0.61	0.49	0	1
Rent	17,462	0.36	0.48	0	1
Occupy	17,462	0.03	0.17	0	1
Avg HH Inc (week)	17,462	1,363	1,341	1.03	9,951

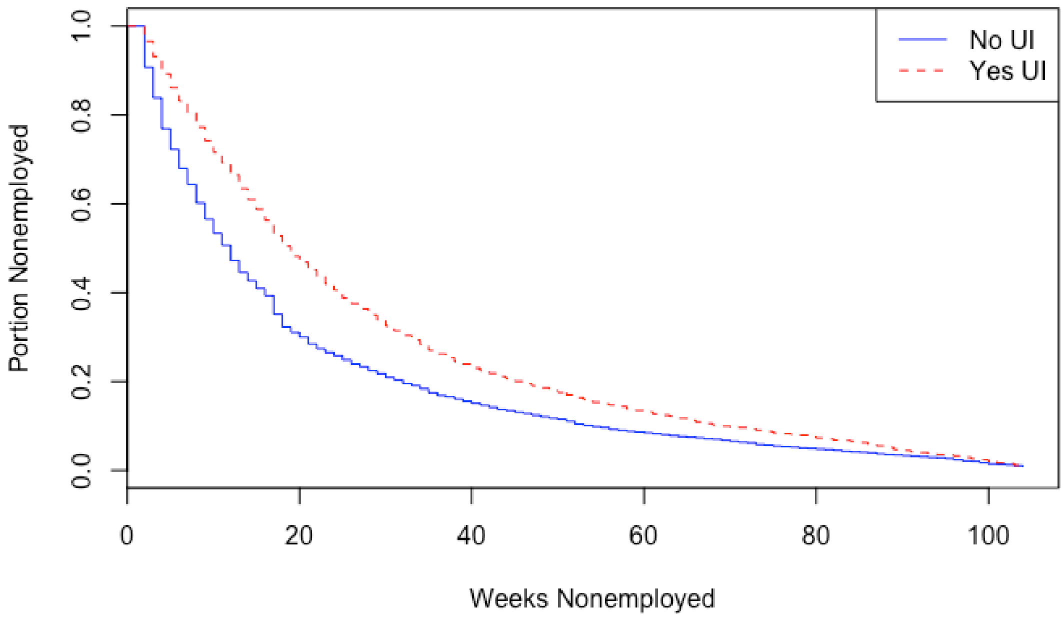
Source: SIPP 1994-2008 panels and author calculation. See text for detail.

Table 3.1, *continued*

Statistic	N	Mean	St. Dev.	Min	Max
Prior Emp					
Duration (weeks)	17,462	54	47	6	267
Avg Earn (week)	17,462	550	563	0	9,921
Avg Hours	17,462	36.9	17.9	0	168
Ever PT	17,462	0.645	0.479	0	1
Ever Multi-Job	17,462	0.324	0.468	0	1
Future Emp					
Duration (weeks)	17,461	52	52.1	1	265
Avg Earn (week)	17,461	543	591	0	14,291
Avg Hours	17,461	36.8	18.9	0	170
Ever PT	17,461	0.6	0.5	0	1
Ever Multi-Job	17,461	0.324	0.468	0	1
Changes					
$\triangle$ Wage	17,461	-1.6	72.7	-266	254
$\triangle$ Log Wage	17,461	-0.3	1.5	-5.6	3.5
$\triangle$ Educ	17,462	0.013	0.114	0	1
$\triangle$ Kid	17,462	0.024	0.152	0	1
$\triangle$ Marital	17,462	0.015	0.121	0	1
$\triangle$ Living	17,462	0.038	0.191	0	1

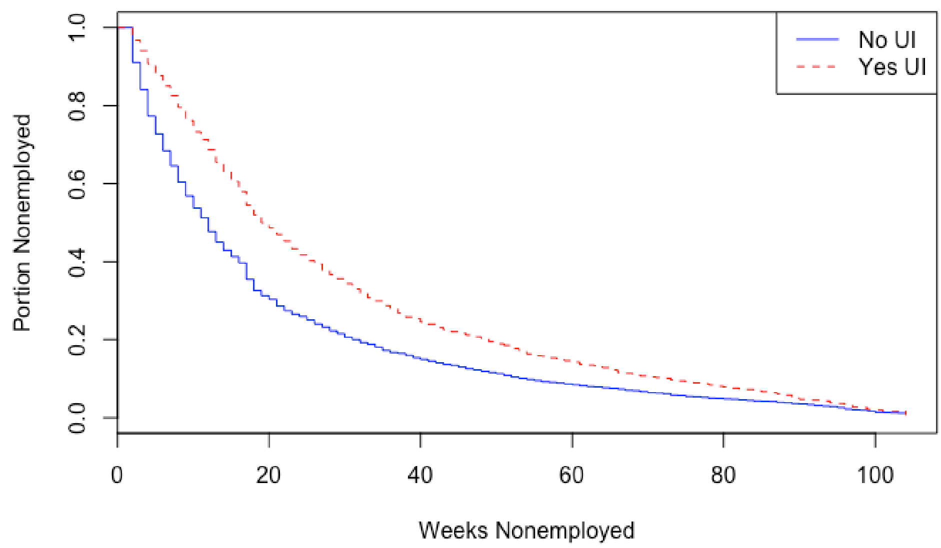
Source: SIPP 1996-2008 panels and author calculation. See text for detail.

Figure 3.1: Kaplan Meier Survival Curves, Recipient vs Non-Recipient

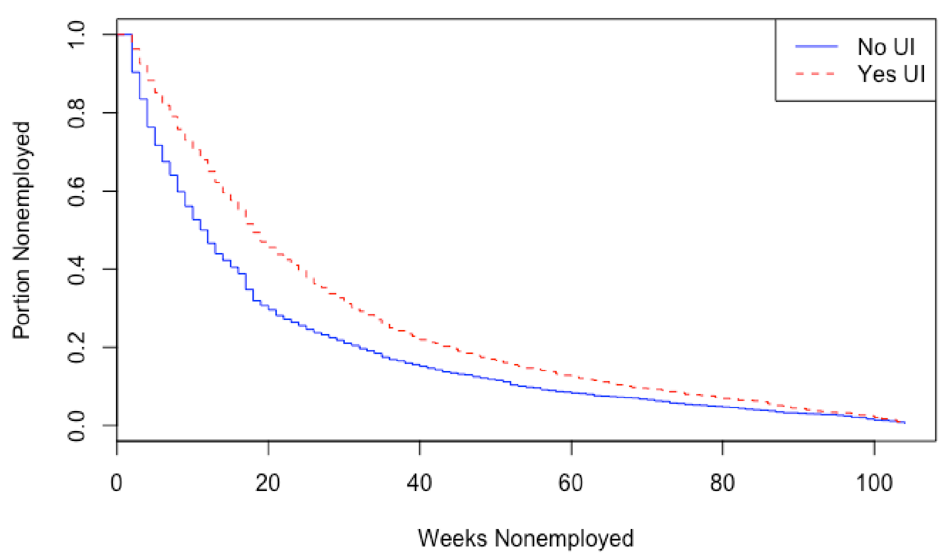


Full sample of nonemployment spells.

Figure 3.2: Kaplan Meier Survival Curves, High vs Low Potential Duration States

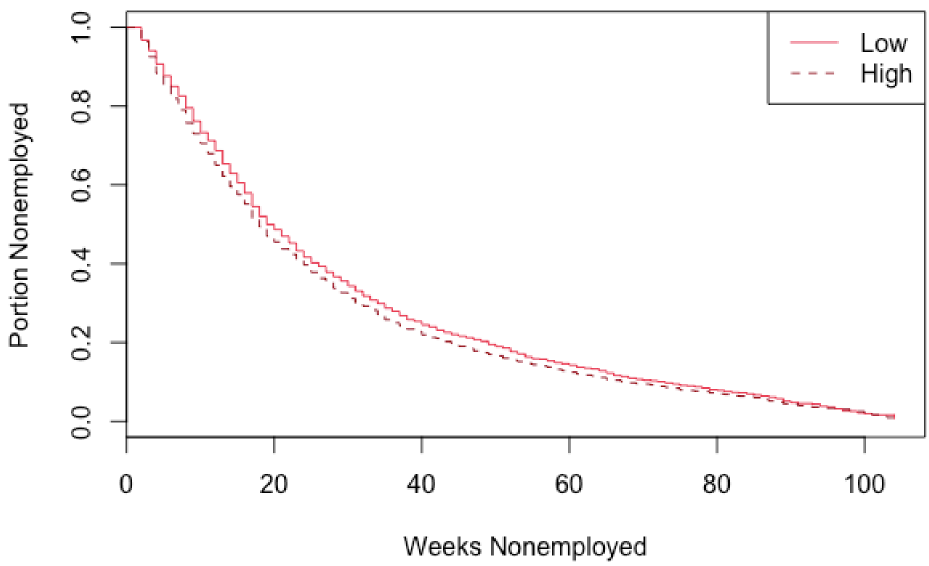


(a) Low Potential Duration States

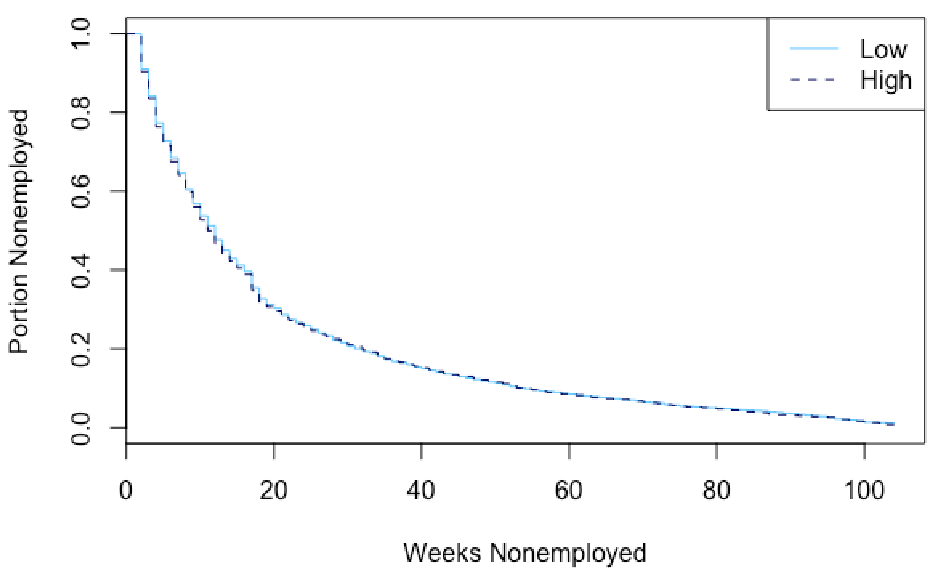


(b) High Potential Duration States

Figure 3.3: Kaplan Meier Survival Curves, High/Low, Recipient/Non-Recipient



(a) UI Recipients in High and Low Potential Duration States



(b) Non-Recipients in High and Low Potential Duration States

Table 3.2: Wage Difference Regression Results

	(1)	(2)	(3)
Receive UI	−0.0436	0.0006	−0.0044
	(0.0281)	(0.0308)	(0.0310)
Log Prior Earn		0.0150	0.0164
		(0.0168)	(0.0167)
Log Avg Week Ben		−0.4691***	−0.9630***
		(0.0579)	(0.0892)
Avg Pot Dur		0.0140***	0.0184***
		(0.0047)	(0.0064)
Female		−0.0322	−0.0292
		(0.0283)	(0.0283)
Age		−0.0233***	−0.0242***
		(0.0080)	(0.0080)
Age2		0.0002	0.0002*
		(0.0001)	(0.0001)
HS		−0.0211	−0.0017
		(0.0394)	(0.0395)
Less HS		−0.0614	−0.0397
		(0.0473)	(0.0479)
Some Coll		−0.0393	−0.0227
		(0.0384)	(0.0384)
Married		0.0180	0.0142
		(0.0388)	(0.0388)
Not Married		−0.0082	−0.0099
		(0.0466)	(0.0465)

Table 3.2 *continued*

	(1)	(2)	(3)
Black		0.0620	0.0680
		(0.0458)	(0.0484)
Asian		−0.0205	0.0046
		(0.0968)	(0.0996)
Other Race		0.0903	0.1239*
		(0.0669)	(0.0680)
Enroll		0.0382	0.0261
		(0.0446)	(0.0446)
Kids		0.0395	0.0422
		(0.0303)	(0.0303)
Own		0.0702**	0.0711**
		(0.0288)	(0.0292)
Urate		0.0325***	0.0677***
		(0.0068)	(0.0084)
Constant	−0.2978***	2.2479***	4.3840***
	(0.0164)	(0.3646)	(0.5199)
State FE			✓
Observations	16,920	16,912	16,912
R^2	0.0002	0.0144	0.0243
Adjusted R^2	0.0001	0.0133	0.0204
Residual Std. Error	12.8840	12.8012	12.7551
F Statistic	3.1100*	12.9998***	6.1771***
Note:			*p<0.1; **p<0.05; ***p<0.01

Source: SIPP 1994-2008 panels. See text for detail.

Table 3.3: Wage Difference Regression Results

	(1)	(2)	(3)
Receive UI	-0.0565^{**}	-0.0072	-0.0117
	(0.0284)	(0.0312)	(0.0314)
Duration (Weeks)	0.0017^{**}	0.0010	0.0009
	(0.0007)	(0.0007)	(0.0007)
Log Prior Earn		0.0149	0.0164
		(0.0168)	(0.0167)
Log Avg Week Ben		-0.4687^{***}	-0.9645^{***}
		(0.0579)	(0.0892)
Avg Pot Duration		0.0140^{***}	0.0183^{***}
		(0.0047)	(0.0064)
Female		-0.0342	-0.0310
		(0.0283)	(0.0283)
Age		-0.0237^{***}	-0.0245^{***}
		(0.0080)	(0.0080)
Age2		0.0002^{*}	0.0002^{*}
		(0.0001)	(0.0001)
High School		-0.0234	-0.0038
		(0.0395)	(0.0396)
Less High School		-0.0651	-0.0429
		(0.0474)	(0.0480)
Some Coll		-0.0402	-0.0235
		(0.0385)	(0.0384)
Married		0.0195	0.0154
		(0.0387)	(0.0387)
Not Married		-0.0078	-0.0096
		(0.0466)	(0.0465)

Table 3.3 *continued*

	(1)	(2)	(3)
Black		0.0594	0.0663
		(0.0459)	(0.0485)
Asian		−0.0235	0.0021
		(0.0968)	(0.0997)
Race Other		0.0887	0.1226*
		(0.0670)	(0.0681)
Enroll		0.0225	0.0121
		(0.0462)	(0.0461)
Kids		0.0393	0.0420
		(0.0303)	(0.0303)
Own		0.0707**	0.0716**
		(0.0288)	(0.0291)
U Rate		0.0316***	0.0669***
		(0.0069)	(0.0084)
Constant	−0.3264***	2.2463***	4.3964***
	(0.0203)	(0.3646)	(0.5197)
Other Controls			✓
State FE			✓
Observations	16,920	16,912	16,912
R^2	0.0007	0.0146	0.0244
Adjusted R^2	0.0006	0.0134	0.0204
Residual Std. Error	12.8812	12.8006	12.7547
F Statistic	5.7387***	12.4730***	6.1157***
Note:			*p<0.1; **p<0.05; ***p<0.01

Source: SIPP 1994-2008 panels. See text for detail.

Appendix A

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